
Bronze Age World System Cycles¹

by Andre Gunder Frank

This essay explores the geographical extent of the world system and dates its cyclical ups and downs during the Bronze Age and, in a preliminary way, the early Iron Age. The scope of these twin tasks is exceptionally wide and deep: wide in exploring a single world system that encompasses much of Afro-Eurasia, deep in identifying systemwide economic and political cycles since more than 5,000 years ago.

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1. It is a pleasure to acknowledge the help of all the many authors whom I have quoted so much above, among them especially Mitchell Allen, Christopher Edens, Barry Gills, and Shereen Ratnagar, who additionally sent me very detailed written comments on a previous draft. They will recognize my input of their help but perhaps not much of their criticism in this revision. Several friends and particularly Philip Kohl, Kristian Kristiansen, and Andrew Sherratt also helped enormously by making not only their own but also their friends' unpublished manuscripts available to me.

The world system and its cycles have long determined the economic, political, and cultural opportunities or limitations faced by regions, peoples, and their political institutions and leaders. Cyclically alternating global warming and ice ages probably also affected economic and political fortunes. Even today, a rising economic sea lifts most boats even if some capsize. A receding world economic tide or stormy weather sinks many more ships of state, but the same crisis that generates their decline offers new opportunities to (literal) upstarts elsewhere. The historical review offered here will show that political-economic fortunes and hegemonic rivalry and its outcomes were already being vitally affected by participation in a world system in the Bronze Age. Detailed demonstration of how the system operated is left for another time and/or to others better qualified than I. It may be useful therefore to attempt to state at the outset what is and what is not proposed here and to anticipate and answer some objections to both.

The first objection may be that the task is impossible to accomplish. In particular, it may be rightly argued that I lack the professional training or experience in archaeology and history for it and have insufficient knowledge of the area, the period, the materials, and the problems and pitfalls of their study. My use or citation of particular facts, sources, and/or "authorities" may also appear objectionable on the grounds that (supposedly or perhaps even really unbeknownst to me) they have been discredited by "the profession." Another objection (or perhaps another version of the same one) is that even the best archaeologists and historians today lack the factual evidence and analytical methods necessary to establish or even indicate the extent of such a world system and its cyclical ups and downs. My perhaps insufficient answer is that fools rush in where archangels fear to tread. It is not that I can claim to know better, but perhaps in knowing less about the obstacles as well and bringing the fresh and unencumbered perspective of an outsider to the task I am more willing and perhaps even able to try. Thus, I make bold to propose a new outline of the world system and older datings of its cyclical rhythm than have others heretofore. In doing so, however, I can challenge others more competent than I to test and revise my tentative findings.

A second objection will be that there was not *one* world system in the Bronze Age but, if any, many. Even by the criteria of participation in a single system that I shall set out below, there probably were several such "systems" in Bronze Age and later times, and certainly none of them was world-encompassing. There is, however, increasing evidence that one such world system did unite a vast array of regions and peoples in a common historical process. Apparently this world system was centered on West/Central Asia and the Eastern Mediterranean/North Africa but extended far beyond this. Moreover, it was this central world system (as Chase-Dunn and Hill [1991], combining the designations of Gills and Frank [1990, 1991] and Wilkinson [1987], suggest that we call it) that eventually expanded

to incorporate all the rest of the world into a world system that now does include one and all.

A third objection will be that, even if the existence of such a world system as early as the Bronze Age were admissible, it could hardly have experienced simultaneous systemwide cyclical phases of rapid economic expansion and subsequent contraction or slower growth. Even today, however, some economic sectors (micro-electronics, biogenetics) and regions (in East Asia) are out of step or phase with the systemwide economic crisis, but only an ostrich-like head-in-the-sand view can deny the existence of that crisis. I shall marshal evidence that something at least analogous can be identified as far back as the early Bronze Age.

Other objections may focus on my failure to pursue related inquiries into more conventional questions such as ecology, technology, state formation, class structure, language, race, culture, and religion. At a time of nearly worldwide assertions of "ethnicity" and diversity, a statement of world-system unity in diversity may also seem to committed activists "politically incorrect." Social theorists may find especially lacking a theoretical analysis of how the relations among all of these and other factors "make the system tick." I do not deny the importance of these and other internal/local/national/societal, institutional, cultural, and voluntarist/agency factors. However, those who emphasize them in practice and theory to the exclusion of the real world-systemic and cyclical "outside" forces beyond them do so at their peril. This is because the latter determine the opportunities and limitations of the former. Therefore, all these and other more conventional sociopolitical and cultural concerns and theoretical problems will only be touched on in the text and/or relegated to at best some suggestive questions and answers in the conclusion.

Nonetheless, I shall begin with a brief attempt to place the present inquiry in the context of certain ongoing discussions. One of these discussions is about the nature of and/or the appropriate approach to the study of the "ancient economy," in which primitivists and substantivists have locked horns for generations with modernists and formalists. Placing the apparently "economicist" approach adopted here in this context will perhaps make it more defensible. A second discussion is the more recent one on whether there was one world system or several and how to study the same. A brief review of this discussion will also offer occasion to set out the criteria for identifying this world system. A third discussion focuses more narrowly on previous versions of the 5,000-year-world-system thesis (Frank 1990, 1991a; Frank and Gills 1992, 1992-93, 1993; Gills and Frank 1990, 1991, 1992) and the controversy and independent attempts at empirically grounded tests of its long-cycle datings that the thesis has elicited. This essay is an extension and revision of the most recent work including empirical tests of its cycle datings and additional evidence for the Bronze and Iron Ages. It also attempts to push the identification of this succession of cycles more than another millennium back into the 4th millennium B.C.

Hard evidence on such (system-) widespread alternating phases of more rapid and slower economic expansion, contraction, and/or crisis is, of course, hard to come by. To my knowledge, prior to Gills and Frank (1992) no one had even attempted any such assembly of evidence as is presented below. For economic upswings, I examine evidence or at least assertions regarding various regions in Eurasia of economic expansion of production and/or trade, population growth, increase in city size, and even diplomatic missions. Conversely, for downswings or crises, I seek evidence or assertions of absolutely or relatively reduced production and/or trade, population decline or reduced growth, decline in city size, and abandonment of cities.

For instance, I will draw on tests of the Gills and Frank (1992) dating of cyclical phases independently performed by researchers using data on changes in city size. However welcome these are, their reliability may be compromised by (1) my own interpretation of (2) their interpretation of (3) their source Chandler's (1987) interpretation of (4) Chandler's sources of city-size data, which (5) are incomplete and (6) may be erroneous and probably record more and greater city sizes in West Asia than in East Asia—all of which may be subject to still other, unidentified problems, among them the use of the city-size measure because it is more readily available than others. Thus, reliance on city-size and other data or assertions is not meant to suggest that they are all definitively reliable but only that I am doing the best I can with every little bit that may help.

More often than not, also, I must compare, contrast, and combine statements by others who have observed economic growth or decline here and there to try to get a picture of a world-systemwide pattern and sequence. Sometimes, direct economic evidence of expansion and/or contraction is not readily available, and I must try to infer it from recorded social or political events—for example, the rise and decline of empires, "civilizations," political (in)stability and war/peace, and hegemonic power/intense rivalry. Of course, the evidence, my and others' interpretation of the same, and especially my inferences are open to doubt and critique—and to improvement!

I draw on this information below in an attempt to reassemble the jigsaw-puzzle picture of the world system in the Bronze Age. However, this jigsaw-puzzle assembly differs from the usual kind in several ways that make it much more difficult: (1) the number of pieces in "the box" is indeterminate, indeed infinite (if cut small enough), and it is possible to place or assemble only a few of them here. (2) There is no original design or intended final picture on "the box" to guide the assembly. (3) It is impossible to follow the usually easier procedure of defining the outer margin of the picture with pieces that have at least one straight edge. In this case, on the contrary, it is the very outer margin or extension of the world system that is most difficult to identify. Instead, it seems easiest to begin with some pieces that appear to be in the better-known "core." (4) The task is not a one-time enterprise. The shapes of the pieces and their

(core-periphery and hegemonic) fit with their neighbors change constantly over time. Perhaps this change is near-random; perhaps it occurs in cycles that should also be identified. (5) One the principal tasks, and here the main one, is to identify such cycles.

Of course, our picture of the world system depends on the survival of textual and the excavation of archaeological evidence. Of course also, archaeologists encounter untold difficulties in constructing a general picture from individual artifacts. Especially difficult for present purposes is making locally found artifacts reveal identifiable long-distance connections and suggesting how important or persistent they were. Moreover, beyond the vagaries of what did and did not survive, the pattern of archaeological digs and their analysis is a function of present-day economic, cultural, and political vagaries. Thus, Kohl (1984a) remarks, for instance, on the Soviet focus on sites rather than regions (to which the work of E. N. Chernykh is a remarkable recent exception) and their preferential access to sites within the (former) Soviet Union. This allows regions to the south thereof to fall through today's political-economic cracks, however important their earlier participation in our world system. Elsewhere as well, contemporary economic, political, cultural, or other reasons result in the neglect of some historically more important sites in favor of others of less historical significance. Another source of bias is my own "selection" of the evidence. Practically, in two senses of the word, my choice of pieces of the jigsaw puzzle is largely based on the documentation kindly supplied to me by my friends from their own and others' writings. All these and other factors undoubtedly introduce gaps and/or distortions into the archaeological and historical record. Moreover, the pieces I have selected tend to be more often "economic" (trade) than "political" (warfare), "social" (migration), and/or "cultural" (diffusion), and the archaeological record (or at least the documentation available to me) more readily permits the assembly of West Asian pieces—and, even then, only very few of these pieces can be assembled here.

The "Ancient Economy" Debate

In the debate about the "extent of the market" (to recall Adam Smith's phrase that related it to the "division of labor") in the ancient economy, Edens and Kohl (1993) distinguish the following positions: among historians, the primitivists, such as Weber (and more recently Finley [1985]) vs. the modernists, such as Meyer; among anthropologists, the substantivists, headed by (the nonanthropologist!) Karl Polanyi (1957, 1977) and his defenders, such as Dalton, joined by Renfrew and his followers among archaeologists who saw some down-the-line trade, vs. the formalists, such as Le Clair, Herkovits, and even Firth. In opposition to the modernist/formalists, who argued that modern economic analysis was applicable to the ancient economy, the primitivist/substantivists (and, indeed, Marx before them) denied the importance of market relations, capital accumulation, and long-distance trade in the ancient world.

Their position was challenged long ago, at least *de facto*. Among historians, Weber's contemporary Werner Sombart (1967, 1969) did so, and then so did the archaeologist Childe (1936, 1942). Yet, according to Kohl (1987*a*), even Childe "consistently underestimated" the strength of the opposite case. Moreover, in a posthumously published essay even Polanyi wrote that "throughout, the external origin of trade is conspicuous; internal trade is largely derivative of external trade," the motive being either status or profit (Polanyi 1975:154, 136–37). In the 1970s, archaeologists rejected the Polanyi/Finley view and offered reinterpretations of the increasingly available data (Adams 1974, Lamberg-Karlovsky 1975). About the same time, Crawford (1973:273) observed "increasing evidence for private ownership of land, property and therefore capital" and suggested that temples may have acted as banks.

Since then, the empirical and analytical refutation of the primitivist/substantivist argument has been almost unceasing. The evidence—primarily from archaeological finds—has been substantial. The related arguments about the importance in and for the ancient economy of long-distance trade, market relations, demand- and supply-related price formation, monetization, entrepreneurship, and capital accumulation have been so overwhelming that we can at best only point to some of the tip of the iceberg. Among the more conceptual writings are those of Ekholm (1980), Ekholm and Friedman (1982), Rowlands, Larsen, and Kristiansen (1987), Kohl (1987*a*, 1989, 1991), and Edens and Kohl (1993). The more empirical reviews include those of Dales (1976), Ratnagar (1981), Lamberg-Karlovsky (1975), and too many others to recount. (Edens and Kohl [1993] point to Powell 1977, Foster 1977, Gledhill and Larsen 1982, and Zagarell 1986.)

Lamberg-Karlovsky (1975), for example, rejected the Polanyi position on the role of "profit, price-fixing, wholesaling, supply-demand, or even private ownership of land for surplus production. . . . It is the central thesis . . . that all of these existed in a market network at least by the end of the fourth millennium in Mesopotamia." Many records from the 3d and 2d millennia attest to fluctuations in the prices of gold and silver relative to each other and, in both the long and the short term, to land, slaves, grains, oil, and wages (which also changed relative to each other). If these price changes did not respond directly to supply and demand, they did so through administered prices, which also had to respond at least politically to supply and demand. Moreover, "evidence is abundant of the accumulation of human and material capital, including circulating capital not directly involved in the production process . . . and fixed capital" (Silver 1985:163). Documentation from late 3d- and early 2d-millennium Mesopotamia analyzed by Larsen (1987) suggests that public and private accumulation were both complementary at one time and alternatives over time—and, I might suggest, varied with less and more prosperous phases of the economic cycle.

The Polanyi/Finley view was challenged not only regarding local market relations but also regarding long-

distance trade. Adams (1974:247, 248) reported "little doubt that long-distance trade was a formidable socioeconomic force" and that "we have wrongly deprecated the entrepreneurial element in the historical development of at least the more complex societies." Moreover, as Kohl (1989:228) remarks, "the intercultural trade that developed between resource-poor southern Mesopotamia and resource-rich highland areas of Anatolia and Iran necessarily transformed the productive activities of all societies participating in the exchange network without the development of an overarching polity or empire."

Of course, the primitivist/substantivists will not be persuaded by yet another statement of the opposite position (nor should they be convinced by any mere *statement*). Indeed, even those closer to my own position may find it rather too extreme. Moreover, as one of them has suggested, I may be confounding a statement about "reality" with my choice of a conceptual approach to that reality. I do not wish to argue that the "market" existed independently of other institutions in the Bronze Age—or, for that matter, in our own. I only wish to take a position in this ongoing debate, within which I also situate the inquiry below, and I wish to go a step beyond it to insist that "world market" forces also impinged on local institutions and policy formation then as now.

Conceptualizing Center-Periphery and World System(s)

Some more conceptual writers among the modernist/formalists have had recourse to at least some aspects of world systems theory. A new wave in archaeological studies is applying center-periphery and/or world systems analysis to the study of complex societies of the past (see Rowlands, Larsen, and Kristiansen 1987; Champion 1989; Chase-Dunn and Hall 1991; Algaze 1993; Allen 1992; Woolf 1990; and Sherratt 1992, n.d.). A half-century earlier, Childe had already written that if the economy of the Early Bronze Age cities could not expand internally because of the overconcentration of purchasing power, it had to expand externally (Childe 1942:139). The center sought "to persuade their possessors to exchange the needed raw materials for manufactures." According to Childe, this trade was from the beginning a political trade between elites in the center and elites in the periphery in which the center sought to induce the periphery to render up a surplus. This is how he explained, for instance, the commercial ventures and associated military campaigns of the Akkadian King Sargon I in 2350 B.C.

Recent excavation at Habuba Kabira in northern Syria of a southern Mesopotamian colony "represents a deliberate Lower Mesopotamian penetration up the Euphrates . . . to secure direct control of vital raw materials and luxuries from the Syrio-Anatolian regions and to regulate exchange of goods from the east and south-east passing this way" (Moorey 1987:44). Hiebert and Lamberg-Karlovsky (1992:3–4) write:

It is of importance to recognise that the phenomenon of expansion and/or colonisation appears within an *early* context of emergent cultural complexity in various regions of western and central Asia. Territorial expansion, whether colonial, imperial, or military, appears to be embedded in the process of *emerging* state polities.

As examples they refer to Egyptian “colonies” in the Sinai and Palestine in the Naqada III period, Sumerian expansion and “colonies” to the north and on the Iranian plateau in the Middle Uruk period, Proto-Elamite replication of these, also on the Iranian plateau, and the Harappan “colony” at Shortugai in Central Asia. They caution that far more research is needed before we can comprehend the “causes” of this territorial expansion:

Areas of cultural complexity are constantly confronted by both an internal and external competition that extend beyond the need for resources. Increasing energy expenditures for maintaining an administrative bureaucracy, establishing networks of communication, increasing agricultural and commodity production, and sustaining the costs involved in local conflicts, all of which inevitably emerge in efforts toward centralisation, could all lead to expansionist tendencies. . . . Ancient societies were dependent upon human, animal, and plant productivity, so the solution to declining marginal returns could be accomplished by territorial expansion and the exploitation of new resources, land, people, etc. . . . the Central Asian Bronze Age joins the community of Bronze Age civilisations in replicating this process of expanding into a distant periphery.

Nonetheless, they suggest that “conflict, warfare, alliance, and the manipulation of political power” are as important as the “typical” and “universal” explanation in terms of the economic need for trade and control of resources. This may be more the effect than the cause of expansion insofar as the “primary agents” are to be sought in political processes. I am inclined to answer that political processes may well be the direct *agents* but the above-mentioned and other economic imperatives are more likely to be their *causes*.

Many of these recent appeals to core-periphery categories and several world systems or to one world system are, however, only halting. Some engage in seemingly arcane discussions with Wallerstein (1974), who never claimed and indeed denies (1991) that his “modern world-system” extends back beyond A.D. 1450. Thus, Woolf’s (1990) examination of the Roman empire seems to get lost in Wallerstein’s distinction between “world-systems” and “world empires,” which I regard as more misleading than clarifying. Notwithstanding its title, Rowlands, Larsen, and Kristiansen’s (1987) book is replete with assertions about the limitations of core-periphery analysis.²

2. The two last named had, however, become more enthusiastic about world system categories by 1992, when they presented papers in panels with me (and see Kristiansen 1993a, b).

Kohl (1989) invokes the "use and abuse of world systems theory" only to argue that "nowhere in the ancient world may one properly speak of 'world' structures of unequal exchange, of 'world' labor markets, or of economic dependence and underdevelopment" (Edens and Kohl 1993:4). In particular, Kohl emphasizes that manufacturing cores had no special advantages, and especially no technological monopoly, over raw-materials-exporting peripheries and takes pains to deny any "development of underdevelopment" (Frank 1966) in the ancient world (Kohl 1987*a*, 1989, 1992; Edens and Kohl 1993). However, as Ekholm and Friedman (1982:90–91) point out, "center/periphery relations are not necessarily defined in terms of their [raw material-manufactures] import-export pattern. . . . Center/periphery relations refer, rather, to different structural positions with respect to total accumulation" of capital, from which they derive differential advantages and disadvantages. Elsewhere, Kohl (1992) demonstrates that three regions in Transcaucasia cannot be considered in isolation from each other or from Mesopotamia and Persia to the south and regions in European Russia to the north. "Influences, sometimes involving actual movements of goods and peoples, were felt from all directions. But such 'influences' . . . do not constitute evidence for a world system . . . in any Wallersteinian sense" (p. 133). Despite repeated disclaimers, Kohl is among those who best demonstrate the existence, albeit with multiple and shifting cores, peripheries, and hinterlands, of "the West Asian Early Bronze Age world system" (Edens and Kohl 1993:59–60).

This shadowboxing with a nonexistent opponent seems less than fruitful. It seems better just to use world-system categories where and when they can help clarify the "reality" of the ancient world. Gills and I (1992) emphasize that throughout most of history and prehistory there have been sets of interlinked hegemonic cores with their respective peripheries and hinterlands. However, several cores seem to have experienced nearly simultaneous (cyclical?) ups and downs, and their downs have often led to shifts of hegemony to other, sometimes recently emerged, cores. Moreover, hegemony may be defined as a "hierarchical structure of the accumulation of surplus among political entities, and their constituent classes, mediated by force. A hierarchy of centers of accumulation and polities is established that apportions a privileged share of surplus, and the political economic power to this end, to the hegemonic centre/state and its ruling/propertied classes" (Gills and Frank 1991).

Important here is the distinction between various ancient world systems and the *one* central world system. Thus, for instance, Algaze (1993) points to two different Bronze Age world systems in what is now West Asia/the Middle East. Instead, Gills and I (1990, 1991) and Wilkinson (1987) insist that we can identify a single world system in the Bronze Age, differing only in that Wilkinson dates its origin at 1500 B.C. and we see it as well over a millennium earlier. We all agree, moreover, that there is an unbroken historical continuity between the central civilization/world system of the Bronze Age

and our contemporary modern capitalist world system. It is the same system, but, as Gills suggests, it has not remained the same throughout its evolution.

The criterion for identifying a single world system is that no part of this system would be as it is (or was) if other parts were not as they are (or were) (Frank 1990; Gills and Frank 1990, 1991). The interaction between one part of the system and another may be only indirect or chain-linked, and the various parts may all have reacted to and upon the same global ecological constraints. Gills and I (1990:27–28; 1991:84–86) have explained:

The capture by elite A here (with or without its redistribution here) of part of the economic surplus extracted by elite B there means that there is “inter-penetrating accumulation” between A and B. This transfer or exchange of surplus connects not only the two elites, but also their “societies” economic, social, political, and ideological organization. That is, the transfer, exchange or “sharing” of surplus connects the elite A here not only to the elite B there. Surplus transfer also links the “societies” respective processes of surplus management, their structures of exploitation and oppression by class and gender, and their institutions of the state and the economy. Thus, the transfer or exchange of surplus is not a socially “neutral” relationship, but rather a profoundly systemic one. Through sharing sources of surplus, the elite A here and the classes it exploits are systemically inter-linked to the “mode of production,” and even more important, to the *mode of accumulation* in B there. By extension, if part of the surplus of elite B here is also traded, whether through equal or more usually unequal exchange, for part of the surplus accumulated by elite C there, then not only B and C but also A and C are systemically linked through the intermediary B. Then A, B, and C are systemically connected in the same over-arching system of accumulation.

This means that surplus extraction and accumulation are “shared” or “inter-penetrating” across otherwise discrete political boundaries. Thus, their elites participate in each other’s system of exploitation vis-à-vis the producing classes. This participation may be through economic exchange relations via the market or through political relations (e.g., tribute) or through combinations of both. All of these relations characterize the millenarian relationship, for instance, between the peoples of China and Inner Asia. This inter-penetrating accumulation thus creates a causal inter-dependence between structures of accumulation and between political entities. Therefore the structure of each component entity of the world system is saliently affected by this inter-penetration. Thus, empirical evidence of such inter-penetrating accumulation through the transfer or exchange of surplus is the minimum indicator of a systemic relationship. Concomitantly, we should seek evidence that this inter-linkage causes at least some element

of economic and/or political restructuring in the respective zones.

For instance, historical evidence of a fiscal crisis in one state or a zone of the world system (e.g., in third-century Rome) as a consequence of an exchange of surplus with another zone would be a clear indicator of a relationship at a high level of systemic integration. Evidence of change in the mode of accumulation and the system of exploitation in one zone as a function of the transfer of surplus to another zone would also constitute evidence of systemic relations. Evidence of political alliances and/or conflict related to participation in a system of transfer of surplus would also be considered evidence of a systemic relationship. According to these criteria, if different "societies," empires, and civilizations, as well as other "peoples," regularly exchanged surplus, then they also participated in the same world system. That is, "society" A here could and would not be the same as it was in the absence of its contact with B there, and vice versa.

Trade in high-value luxury items, not to mention precious metals in particular, may, *contra* Wallerstein (1974, 1989), be even more important than lower-value staple trade in defining systemic relations. This is because the high-value "luxury" trade is essentially an inter-elite exchange. These commodities, besides serving elite consumption or accumulation, are typically also stores of value. They embody aspects of social relations of production, which reproduce the division of labor, the class structure, and the mode of accumulation. Precious metals are only the most obvious example, but many "luxury" commodities have played a similar role (Schneider 1977). Thus, trade in *both* high-value "luxury" items and staple commodities are indicators of interpenetrating accumulation.

Despite the emphasis on "economic" trade connections to cement the world system, Gills and I (1990, 1991) also explicitly accepted the world system connections established and maintained through recurrent "political" conflict among "societies" emphasized by Wilkinson (1987). The recognition of such conflict as a mark of participation in one world system is all the more important insofar as much of it has been over "economic" resources and control of trade routes. At the same time, trade in metals and/or weapons may increase military capacity, and that in turn may enhance control over sources of economic resources, including trade itself. Moreover, political conflict and shifting alliances have also been the expression of the alternation between hegemony and rivalry within the world system and/or its regional parts.

Thus, as I have suggested elsewhere (Frank 1992, Frank and Gills 1992), the world system at least since the Bronze Age can be characterized as follows:

1. *Contra* Wallerstein (1974, 1991), our now single world system has historical continuity for at least 5,000 years, emerged with its core in West Asia and Egypt, and

then spread to encompass much of Afro-Eurasia (Frank 1990, 1991a, b; Frank and Gills 1992, 1992–93, 1993; Gills and Frank 1990, 1991, 1992). For Wallerstein, the “modern world-system” emerges only with the rise of Western Europe at its core after A.D. 1500 and still includes much less of the world some centuries later. Before that, for him there had been *several* other regional “world-systems” in Eurasia, not to mention in the “new world” before its incorporation into our preexisting world system after 1492. He rejects this notion of a much older Afro-Eurasian world system (1991:192), explaining the difference a hyphen makes: “My ‘world-system’ is not a system ‘in the world’ or ‘of the world.’ It is a system ‘that is a world.’ Hence the hyphen, since ‘world’ is not an attribute of that system.” For us it is, in that the very system whose core emerged in West Asia over 5,000 years ago was not only worldlike then but developed to become world-embracing later.

2. Capital accumulation was the motive force of world system history. Wallerstein (1991), Amin (1991), and others consider continuous capital accumulation the *differentia specifica* of the “modern world-system,” arguing that previous “world-systems” were tributary, with politics and ideology in command. In contrast, we see capital accumulation as having played a (if not the) central role in the world system for several millennia (Frank 1991b; Gills and Frank 1990, 1991).

3. The core-periphery structure familiar to analysts of dependence, especially in Latin America, and the “modern world-system” (see Frank 1967, 1969) is also applicable to the world system before that.

4. Temporary regional and perhaps world-systemwide hegemony alternates with long periods of rivalry for hegemony, and hegemony is associated with cores as analyzed in the literature on the “modern world-system” since 1500 (e.g., Wallerstein 1984, Modelski 1987, Modelski and Thompson 1988). In my view, however, this succession of core hegemony and rivalry in a single world system also shifted around Eurasia for millennia before 1500. Indeed, “of crucial importance is the fact that the fall of the east precedes the rise of the west” (Abu-Lughod 1989:338) within this same world system (Frank 1990b, Gills and Frank 1992).

5. Economic cycles of alternating ascending (A) and descending (B) phases like those of the “modern world-system” (Wallerstein 1974, 1980; Frank 1987a, b) associated with capital accumulation, changes in core-periphery position, and alternating hegemony and rivalry extend back many centuries before 1492. Modern “long” cycles are of the more or less 50-year-long “Kondratieff” type. How far back they go is still in dispute. Modelski and Thompson (1992) count 19 of them, reaching back to nearly A.D. 1000, but they recognize that these probably nested in longer ones. Gills and I (1992) have argued that a pattern of still much longer cycles goes back to at least 1700 B.C. Of course, these much longer “long” cycles may also contain other shorter cycles, including perhaps cycles of Kondratieff-type duration. Going (1992) has identified Kondratieff-type cycles in Roman times that were Roman empire/econo-

mywide. I now believe that we can identify a cyclical pattern of long ascending and descending phases in a single world system back at least through the 3d millennium B.C. Indeed, the synchronization of these phases is a revealing indication of the extent of that world system. That distant parts of Afro-Eurasia experience economic expansions and contractions nearly simultaneously would seem important *prima facie* evidence that they participate in the same world system.

Similarly, for Edens and Kohl (1993:361) "the existence of an ancient world system is postulated by the largely synchronous processes of rise and collapse recorded throughout this area; it is difficult to deny that one here is witnessing historically connected processes." This synchronism suggests to them "the action of an interrelated set of transregional social forces operative over vast regions of western Asia from the mid-3rd through the mid-2nd millennium B.C." (p. 25).

Summarizing, then, we can list the following among the identifiable—and researchable—criteria of participation in a single world system: extensive and persistent trade connections, persistent or recurrent political relations with particular regions or peoples, including especially core-periphery-hinterland relations and hegemony/rivalry relations and processes, and the sharing of major (and minor) economic, political, and perhaps also cultural cycles.

The Identification of Cycles

We should not, of course, expect to find complete synchronization of phases across the entire world system, especially in its Bronze Age beginnings. It seems quite enough to be able to demonstrate or even suggest "substantial" synchronization of economic good or bad times over very wide areas that are usually considered quite independent of each other. Moreover, other world-systemic cyclical characteristics complicate the pattern. Expansion and contraction seem to begin in one part of the world system, usually in its core, and then to diffuse from there to other parts, including core competitors and periphery. Therefore, cyclical decline tends to mean the relative or even absolute decline of the core power. This decline offers opportunities to rivals, even on the periphery of the system, some of which advance both absolutely and relatively and perhaps even replace the previous core. Incipient exploratory expansions of the world system tend to occur during periods of contraction, and the new areas become the basis of subsequent major investment (Frank 1978a). These out-of-phase elements complicate the identification of past systemwide cycles, especially in the distant Bronze Age, but this is not to say that there were no such cycles.

Gills and I (1992) have identified the alternating phases (A, expanding; B, contracting) of world system cycles from 1700 B.C. to A.D. 1700 as follows:

B: 1700–1500/1400 B.C.

A: 1400–1200 B.C.

- B: 1200–1000 B.C.
 A: 1000–800 B.C.
 B: 800–550 B.C.
 A: 550–450 B.C.
 B: 450–350 B.C.
 A: 350–250/200 B.C.
 B: 250/200–100/50 B.C.
 A: 100/50 B.C.–A.D. 150/200
 B: A.D. 150/200–500
 A: A.D. 500–750/800
 B: A.D. 750/800–1000/1050
 A: A.D. 1000/1050–1250/1300
 B: A.D. 1250/1300–1450

The latest two millennia are reviewed separately and with reference to the emergence of Europe with little change in Frank and Gills (1992–93), with reference to Central Asia in Frank (1992), and extended to the present day with reference to Latin America in Frank (1993).

Wilkinson (1992) and Bosworth (1992), independently of each other, have since tested and confirmed the existence and most but not all of the timing of these cycles, both using data from Chandler's (1987) census of growth and decline in city sizes. Sherratt and Sherratt (1991a) have dated periods during the 2d millennium B.C. coinciding almost exactly with our phases. Kristiansen (1993a, b) has similarly dated expansions and contractions in Europe during the 1st millennium B.C., and Randsborg (1991) has done so for the 1st millennium A.D. I shall bring all these and more to bear on an attempt to refine and where appropriate revise the cycle phase datings we have proposed. Doing so may involve sacrificing attention to the "hegemonial shifts" that we emphasized in principle but shortchanged in practice. In compensation, perhaps, I will try to pay more attention to war. Melko and Wilkinson (1992) have made a convenient tentative accounting of alternating periods of more war and more peace. (Other, more extensive surveys of war perhaps should but here will not be used.) It may be useful to compare these with phases of economic expansion and contraction. I have elsewhere (1987a) distinguished between A-phase (expansive) and B-phase (defensive) wars. Since then, Goldstein (1988) and others have sought to demonstrate that for the past 500 years major (that is, large-scale, "world") wars have occurred at the ends of the A phases of Kondratieff 50-year cycles. Still, taking a longer view, there is theoretical ground for arguing that the greatest incidence of wars—and also of large-scale migratory invasions—should occur during B phases, that is, when enhanced competition for a smaller economic pie generates more military conflict, both internal/national and external/international. Additional evidence on the Bronze Age world system and its cycles includes that of Dales (1976) on Iranian-Indus relations and Ratnagar (1981) on "encounters" between the Harappa civilization and points west and, on these points west, Edens (1992) and Edens and Kohl (1993). Chernykh (1992) proposes cycle datings for most of Eurasia on the basis of the research of two generations of

archaeologists in the then Soviet Union. Other references to more or less precise dates of economic expansions and contractions here and there are scattered through the works of various other archaeologists and others. Because of the gap between their expertise and mine, I often prefer to let them speak for themselves by quoting them directly.

Exploring the Extent of the World System in the Bronze Age

Edens and Kohl (1993:58) write, "One must define the spatial parameters of one's world system, while at the same time emphasizing that the procedure is to some extent artificial in that the system's borders were never hermetically sealed and that they expanded or contracted over time. This difficulty affects the analysis." Not surprisingly, the farther back we go in prehistory, the more difficult it becomes to identify the changing geographical extent and temporal cycle of expansion and contraction of the world system with any kind of confidence.

Edens and Kohl (pp. 58–59) go on to identify the spatial limits to which they refer as follows:

Roughly, the area stretches from the eastern Mediterranean in the west to the Indus valley in the east and from southern Central Asia and the greater Caucasus range in the north to the Sudan and Arabian peninsula in the south. . . . Intriguingly and perhaps not entirely accidentally, [a millennium later] the political borders of the Achaemenid empire, including those areas, like mainland Greece, where the Persians expanded, coincide fairly closely with the limits of the West Asian Early Bronze Age world system.

These limits of the Bronze Age world system coincide fairly closely with ours (Frank 1990; Gills and Frank 1990, 1991, 1992; Frank and Gills 1992). We have advanced the thesis that the present world system was born some 5,000 years ago or earlier in West Asia, North Africa, and the Eastern Mediterranean. Our argument is similar to that of Wilkinson (1987), who identified the birth of "central civilization" through the establishment of systemic and systematic relations between Egypt and Mesopotamia around 1500 B.C., but it pushes the date of the formation of the world system back to at least 3000 B.C. by analogy to the upstream confluence of two or more major branches to form a single river. Moreover, we have suggested that already in the 3d millennium B.C. the world system included not only Egypt and Mesopotamia but also the Arabian Peninsula, the Levant, Anatolia, Iran, the Indus Valley, Transcaucasia, and parts of Central Asia. All these regions were in direct "bilateral" or at least indirect "multilateral," systematic and therefore systemic contact with each other.

Marfoe (1987) examines the emergence of the Egypt-Levant-Syria-Anatolia axis and its extension to Arabia,

Mesopotamia, and Iran from the 5th through the 3d millennium:

During a short time around 3000 B.C., apparently sophisticated, complex systems . . . appeared across an area stretching from the Nile and Aegean in the west to central Asia in the east. It is not impossible that these regional developments may represent a loosely integrated and related series of changes.

[p. 25]

[These] may in part be attributable to an interplay between local and external forces. In this regard, one possible effect of their outcome may have been a “primitive accumulation of capital” and its role as a force for such change. Such a conclusion would include a measure of “market forces” in these periods.

[p. 30]

Between the late fourth and third millennia, a faint, highly buffered “market mechanism” may have operated for different periods of time and in different regions along these networks. [p. 34]

Moorey (1987) pursues the 4th millennium trail from Mesopotamia via Syria into Egypt, noting that scholars were already aware of it over a century ago. He analyzes at least indirect mid-4th millennium exchange via Syria of Gerzean-period Egyptian gold for oil, silver, and lapis lazuli from Mesopotamia, which must have received the latter from farther afield. He also asks but on present evidence is unable to answer whether and to what extent this long-distance exchange altered productive activities in Egypt. He notes the power and status that would have followed from control of the distribution of imported luxury items through local exchange networks even before the Gerzean period. Edens (1992) reviews developments in southern Mesopotamia and Elam, the Central Gulf region including Dilmun = Bahrain, southeastern Arabia and Oman, and the Indus region, suggesting that the roots of the Persian Gulf trade presumably lie in the 5th millennium B.C. and that maritime products already appear consistently in the Mesopotamian archaeological record for the late 4th millennium. Certainly by the 3d millennium (Edens 1992:120, emphasis mine),

the Gulf trade represented a material connection between these four regions, and potentially a mechanism by which emerging *conditions in one region effected changes in others*. However . . . Mesopotamian dealings with lands to the east also involved a range of diplomatic exchanges, elite marriages, cultural hegemony, political clientship, and warfare. Together with trade, all these activities defined *center-periphery relations, whose nature and intensity altered as constituent societies changed*

—and presumably vice versa! Referring specifically to Dilmun, Edens (1992:129) suggests that its “political economy [had] a dialectical relationship” with Mesopotamia. Urbanization and political organization in Bahrain were a function of the Gulf trade and in turn determined Dilmun’s demand for cereals from Mesopotamia

and participation in the transshipment supply to Mesopotamia of copper from elsewhere.

Thus, relations among otherwise distinctive but related and therefore presumably mutually affected regions have left marks in the archaeological record from the 4th and even the 5th and perhaps earlier millennia B.C. Many were based on differences and complementarities in natural resource endowments, which generated trade, migration, invasion, and in general diffusion. Kohl (1978:475) asks rhetorically if the "world" system already stretched from the Balkans and the Nile to the Indus in the 4th millennium, and Oates (1978:481) reads the archaeological record to display "international" horizons from at least the middle of the 5th millennium B.C. onwards.

Kohl (1989:227) notes that "profit-motivated trade extended far beyond the political borders of any state and connected . . . [all of these] into a single world system." Foreign trade in the mid-3d millennium, he argues (1978:466), was "an exceedingly complex process, involving the movement of finished luxury commodities, raw materials, and staple products, and was probably conducted both by state agents and by private entrepreneurs. . . . It does show that developments in southwestern Asia were not limited to the alluvial plains and . . . widely separated communities were linked by complex, well-defined exchange networks." The alluvial plains of Mesopotamia are and were notoriously poor in metals and timber, which they had to import from often very distant sources. Following Larsen (1974, 1976, 1987), Allen (1992) draws a map centered on Assur, which imported gold and silver from Anatolia and tin and other metals from Afghanistan, while exporting textiles to both and reexporting Central Asian Afghani and/or Iranian tin to Anatolia. It also imported textiles and perhaps grain, mainly wheat and barley, from southern Mesopotamia and paid for it with gold, silver and other metals imported from Anatolia and Afghanistan. Allen (personal communication, September 1992) points out, however, that he centred his map on Assur not because it was a center—it was a "semi-periphery"—but because we do not know where its center/core was. Mesopotamia, in turn, exported wool, textiles, and grain to various parts of the even more resource-poor Persian Gulf. This region was a fulcrum for and dependent on trade with Oman and with the Indus region, which in turn had connections with Central Asia.

Edens (1990), reviewing the evidence for Indus-Arabian interaction during the Bronze Age, mentions timber, textiles, and foodstuffs and surveys the archaeological record for trade in ceramics, glyptic seals, metal objects, mostly copper/bronze celts or flat axes, stone weights, beads, soft stone vessels, raw materials (mostly only transshipped through Harappan hands), semiprecious stones, ivory, tin, copper, gold, and silver from a wide variety of sources; shell; bitumen asphalt; and biotic forms such as zebu and sorghum.

Ratnagar (1981) concentrates on the trade relations between Mesopotamia and the Indus Valley but also discusses other trade, partly administered and partly free-

lance. Copper came from Cyprus and the Levant as well as Oman, Iran, and Afghanistan. The rarer tin came from Anatolia, the Caucasus, and Iran. Scattered were copper, tin, and gold ores, as well as lead ores, which were sources of silver, the latter rarely available alone and found mainly in Anatolia (Marfoe 1987). Wood came from Meluhha on the Indus, Magan in Oman, and Dilmun as well as the Siwaliks and Punjab. Egypt imported wood from the Levant. Egypt and/or Nubia in turn exported gold, and so did Arabia, Armenia, and probably the Indus Valley. The latter exported timber, copper, gold, ivory, stones, and beads to Mesopotamia and imported food, textiles, silver, and earthenware. Steatite vessels were also traded, apparently both as containers of other goods and as trade items in themselves. Pottery did not travel over very long distances, presumably because of its weight, which made cloth and reed containers more suitable.

Ratnagar also itemizes trade in precious commodities such as ivory, steatite, carnelian beads, dice, bird figurines, conch shells, monkey figurines, pearls, and lapis lazuli (the latter from a single source in Afghanistan). Trade in silver seems to have had a special role as a medium of exchange, unit of accounting, and store of value. Archaeological finds of weights and seals from distant locations also attest to widespread trading networks, using overland, riverine, coastal, and sea transport, individually and in combination. Evidence survives of individual shipments of 20 tons of copper, and exports from Assur to one small town have been estimated at some 100,000 textiles over a 50-year period. A useful summary and mapping of Bronze Age long-distance trade in and between the West Asian and Eastern Mediterranean regions is provided by Klengel (1978). Significantly, the regions referred to here are almost entirely to the south of the east-west mountain ranges that run across much of Asia.

Chernykh (1992) argues, however, that the development of metallurgy was an increasingly interconnected and shared process throughout most of Eurasia north of these ranges and concludes that "the world system itself has turned out to be far more extensive than appeared earlier" (p. 304). He also suggests that "from at least the fifth millennium B.C. until the third millennium B.C., the peoples of the EMA [Early Metal Age] cultural zone seem to have shared the same developmental cycle: the formation and decline of cultures at various levels generally coincided" (p. xxi). In his closing chapter, he returns to "the contemporaneity of the decline and formation of various systems over the vast expanses of Eurasia and the Old World as a whole . . . [when] a whole chain of similar systems arose almost simultaneously, from the Atlantic to the Pacific: the European, Eurasian, Caucasian and Central Asian provinces, along with others outside the U.S.S.R." (p. 302).

In the 5th and 4th millennia B.C. there was already "highly developed commercial exchange" and the export of "huge quantities" of copper and gold from mining and metallurgical centers in Thrace and the Carpatho-Balkan region to oreless consumer regions in Moldavia

and the western Ukraine and on the Dnieper and the Volga (p. 50). With the significant drop of metal production in the early Bronze Age, the "disappearance [of this complex] . . . was as unexpected as its appearance" (p. 51). Kohl (1984a) suggests that the archaeological record bespeaks some form of prehistoric "silk route" connection with China 2,000 years before the classical one. Chinese scholars also refer to the same even earlier (see Frank 1992a).

Chernykh (1992) examines interconnected but shifting predominantly east-west trade nets involving metals and their products across much of Eurasia north of the mountain ranges. Elsewhere (1991:36) he writes of an abundance of metal finds in sites thousands of kilometers from the sources. However, and despite the political restriction of his research to the former Soviet Union, he also offers at least glimpses of earlier north-south trade relations with Anatolia, Iran, and Afghanistan across the mountains. These, in turn, would also have linked the northern Eurasian trading and migratory system into the West Asian, Gulf, Arabian, and North African ones.

Do we then have two world systems here, the more "traditional" one south of the mountains and the one Chernykh claims to see north of them? Or were they part and parcel of a single one? At least two kinds of evidence support the latter view: the evidence of extensive and recurrent north-south trade, migration and invasion, and cultural/technological diffusion across what Chernykh (1991:35) calls the "Caucasian bridge" from Anatolia eastward and the substantial coincidence of timing of the economic cycles we have identified for the south and those Chernykh has established for the north. This temporal coincidence may be traceable to ecological and/or other systemic commonalities, but it is not very likely to be the result of chance. There is, then, evidence for the existence of *one* immense Afro-Eurasian world system in the early Bronze Age. One of the important tasks of research and analysis is therefore to inquire into its origin and to explore its (cyclical?) expansion and transformation over time.

Much of the development of the outlying civilizations in China, India, Persia, Mesopotamia, Europe, and so on, can only be accounted for in terms of their relations with the peoples of Central Asia, many of whom, moreover, migrated into East, South, and West Asia. I have argued that instead of regarding Central Asian waves of migration, often also bearing advances in productive and warfare technology, as intrusions on the surrounding "civilized" societies, we should consider the possibility that the "pulse of Asia" (to recall the phrase of Huntington [1907]) was at its center (Frank 1992a). Yet the centrality of Central Asia is all too neglected. Much the same may be said, however, of Southeast Asia as the fulcrum of trade, invasion, migration, and cultural diffusion through the Indian Ocean, the South China Sea, and the Pacific Ocean, perhaps already in Bronze Age times but certainly since the Iron Age. Indeed, there is some evidence that with the early-2d-millennium decline of Harappan relations to the west they turned

southward (as Dale suggests) and eastward instead. Friedman (1992) links the latter with the emergence of the trading and migratory system between the Indian East Coast and Southeast Asia and the Lapita expansion into Melanesia and Polynesia. Does this imply their incorporation into this world system already in the Bronze Age?

Bronze Age Cycles in the 3d Millennium B.C. and Earlier

A marked economic decline has been noted in various parts of West Asia from the middle of the 3d millennium B.C. Edens and Kohl (1993:23–24) note that others have also observed “a set of fundamental synchronisms across much of western Asia . . . in eastern Iran, Central Asia, and the Indus . . . in the mid-3rd millennium B.C.” and “the collapse of these expressions of urban complex society, now extending throughout Iran, into the Gulf and, in a modified way, also southern Mesopotamia, by the opening centuries of the 2nd millennium B.C.” Kohl (1978) quotes Oppenheim’s (1954:12) observations about Ur to the effect that in the late 3d millennium “a process of gradual and slow restriction of the geographical horizon marks the entire development of commercial connections. We may well assume that the frequency and intensity of contact had reached a peak early in the third millennium B.C.” Jawad (1965) insists on the ecological, economic, social, and political differences of northern from southern Mesopotamia at this time, but these were apparently insufficient to exclude the north from this same (cyclical?) process. According to Kohl (1984a:242),

“International” relations changed over the greater Middle East during the first half of the third millennium with the collapse of the proto-Elamite “hegemony” in southern and Central Iran . . . according to archaeological evidence from Central Asia, Baluchistan, southeastern Iran and the Indus Valley . . . across the Iranian plateau, in the Gulf area (particularly the Oman peninsula), Mesopotamia, the Anatolian plateau and the Caucasus. . . . But it is unclear what happened to foreign relations in the later third and early second millennia with the collapse of Akkadian rule and the subsequent rise of and demise of the highly centralized Ur III dynasty. Dales (1977) explained the collapse of proto-urban settlements throughout the Indo-Iranian borderlands (during the so-called urban phase) as due to the cessation of long-distance overland trade and development of direct maritime trade between Mesopotamia and the Indus Valley. His theory only represents an unproven hypothesis but deserves serious consideration.

Dales (1976) finds a definite rhythm in trade patterns between the Iranian Plateau and the Indus Valley in the 3d millennium B.C. Strong trade and cultural interaction between Central Asia, northern Pakistan, eastern Persia,

and points west characterized the first half of the millennium. For instance, Turkmenian Altyn Tepe flourished but then declined. At the same time, the Afghan Sistan site of Shahr-i Sokhta was destroyed and abandoned, if only because the main stream of the Helmand River changed course; its Helmand Valley civilization had "totally collapsed by 2500 B.C." However, the reasons may also have been transregional. Dales refers to widespread abandonment of sites from Central Asia through Sistan, southern Afghanistan, and northern Baluchistan and an almost total break in trade routes and spheres of interaction across their trade routes and then a shift also of maritime trade patterns around 2500 B.C.

These observations would imply a half-millennium-long A phase during the first half of the 3d millennium followed by a half-millennium-long B phase. Can we refine these 3d-millennium phase lengths and dates to make them more consistent with the approximately one-quarter-millennium-long A and B phases that we have observed from near the beginning of the 2d millennium? To start with, it may be important to pursue Dale's observation that the cycle phases seem to have moved from west to east. However, the phase marker was not absolutely synchronous but earlier in West Asia than in Central Asia and, finally, the Indus Valley. Though Dales does not say so, this phase marker seems to begin even farther west and earlier, namely, in Egypt. Thus, there may have been a shorter (half-millennial) cycle whose phases were not altogether synchronous over the entire area. Moreover, if this cycle was already operational in the 3d millennium, then it may also have existed, at least incipiently, in the 4th millennium B.C.

Perhaps the first two or more centuries of the 3d millennium might be regarded as an A phase. However, the period 3000–2800 B.C. saw yet another of the 200-year-long waves of migration out of Central Asia that have been noted by Gimbutas (1980, 1981), among others, and Chernykh (1992) and I (Frank 1992a) associate these waves with B phases of world-systemwide crisis. Albeit referring only to the Scandinavian region, Kristiansen (1982:260) notes "disintegration" and the cessation of interregional exchange between 3200 and 2800 B.C. He also suggests, however, that what he calls the "dominant trends" of several "time-space cycles" at the local level "constitute a regional cycle" and that several such regional cycles in turn "may constitute dominant interregional or 'global' cycles" (p. 262). Chernykh (1992:305) notes destabilization throughout the Early Metal Age communities farther north in Eurasia during the second quarter of the 3d millennium. The 27th to 25th century was a time of "major culture-historical change . . . reflected in various spheres (political, ethnocultural, productive and technological), themselves clearly interrelated" (p. 98). Change was manifest in western Asia, Asia Minor, and the more northerly regions of the Circumpontic area. "The period of greatest disruption was probably the twenty-sixth to twenty-fifth centuries B.C. (on the basis of a series of calibrated radiocarbon dates)" (p. 305). Edens and Kohl (1993) note that in the Indus area massive urban growth occurred from 2600 to 2500

B.C. and even more spectacular decline around or after 2000 B.C. Urbanization in southern Turkmenistan from 2600 to 2200 B.C. was followed by population shifts or dispersion eastward from there and other Transcaucasian areas.

Ratnagar (1981) summarizes the "relative chronology of the third millennium" for Barbar, Umm an-Nar, Shahdad, Yahya, Shahr-i Sokhta, Bampur 5–6, Kulli, and Harappa. The period of maximum recorded activity begins between the Mesopotamian periods Early Dynastic I and II, in 2750 B.C., for every site/region except Shahr-i Sokhta and Harappa, where it begins about one to one and a half centuries later. These periods of activity end mostly within the Mesopotamian Early Dynastic III. In five of these instances, the fall-off date is between 2450 and 2350 B.C., the latter for Kulli already in the Akkadian period. For the others, mostly farther east, the period of decline falls between the Akkadian and the post-Akkadian period, around 2250 B.C. Only Harappa, still farther east, continues until 2000 or perhaps even 1800 and Barbar on the Gulf until 1800 B.C. However, "the archaeologically attested trade contacts of the Harappa and Mesopotamian civilizations are the most numerous in the ED III and Akkadian periods," from 2500 to 2250 B.C. (Ratnagar 1981:204). Elsewhere Ratnagar (n.d.) notes "dramatic" declines of both the sown area and the yields of wheat in the Lagash area of Mesopotamia beginning between 2400 and 2100 B.C. and continuing until 1700 B.C. She also notes the time of troubles in Egypt from 2250 to 2035 B.C., when famine and foreign incursions made every Pharaoh's hold on power short-lived. Since the invaders included especially Libyans, the implication is that they were included in or entering this world system.

Ekholm (1980:165) has observed that

in the period around 2300–2200 B.C. there occurred serious economic crises that affected much of the Middle East and the Eastern Mediterranean. Everywhere there is indication of decline in quality and quantity of production that was usually state monopoly and oriented to export. Correlatively, there was an increase of local violence often culminating in obliterating warfare and destruction. These large-scale crises are often explained by barbarian invasions, but it is just as likely that the violence is internal, the only migrants being "capital" and labor forced out of their homes by acute survival problems. The collapse of "supralocal" space leads to accelerating competition between and within political units, that is, to warfare and intensified class struggle.

Melko and Wilkinson (1992) note periods of heightened warfare in and around Mesopotamia in the 27th, 25th, and 23d centuries (but none yet in the other regions). Urbanization in southern Afghanistan also culminated after 2500 and abruptly disappeared after 2000 B.C. Settlement in southwestern and southern Iran peaked around and/or declined after the second half of the 3d millennium. In and around Oman on the Arabian penin-

sula peak copper production, best documented at the Maysar I site, was late in the 3d millennium.

These scattered pieces of chronological evidence may not suffice to identify systemwide cycles during the 3d millennium. All attest only to something of an A phase during the first half and a major B phase spreading from west to east throughout the West Asian world system in the late 3d millennium B.C. Shennan (1993) also finds cyclical increases or decreases in population and occupation which last 500 years or more each from the late 4th to the late 3d millennium in Central Europe. However, much but not all of Central Europe and the Danube Valley experienced (B-phase?) major population decline in the first half of the 3d millennium. These variations largely coincide over so wide an area that he regards it as unlikely that local factors were responsible. On the other hand, he also identifies regional trend variations and even opposites (that is, A phases) among regions.

Yet it seems important to try also to suggest some sort of shorter cyclical pattern, even if only in a tentative way and to incite others to refine and revise it. As Kohl reminds us, "it is important to realize that there is no universally accepted or orthodox Soviet dating system" (Kohl 1981:xxviii). Elsewhere as well, "3rd-millennium dating . . . changes a lot from year to year, depending on who has found what most recently" (Mitchell Allen, personal communication). I shall nonetheless hazard some kind of cycle dating scheme with these and other cautions in mind. On the basis of the evidence reported above, I would suggest a B phase in the 27th to 26th centuries to the west and the 26th to 25th centuries to the east. That the 26th and 25th centuries witnessed some recovery is suggested by the marked activity Ratnagar reports in the more easterly regions beginning around 2750 B.C. and ending mostly around 2400 B.C., but Melko and Wilkinson register increased warfare in Mesopotamia in the 25th century. Edens writes me (personal communication, August 25, 1992) that the evidence suggests a 3d-millennium A phase, at least for western Asia, beginning around 2600/2500 B.C. Urban growth in the Indus Valley and trade between it and other regions also expand soon thereafter. However, Edens finds the end of this phase rather "arbitrary." Nonetheless, I would suggest that another B phase may have begun after 2400 (per Ratnagar) and/or 2300 (per Ekholm). The 23d century again had heightened Mesopotamian warfare in the Melko and Wilkinson table, and this B phase would seem to last until toward the end of the 3d millennium. Unfortunately, Chandler's (1987) data on the number of cities and their sizes as recorded by Bosworth (1992) and Wilkinson (1992) are quite inconclusive for this early period. Wilkinson follows Gills and me (1992) in beginning with the 2d millennium. The data extend to 2250 B.C., with eight cities in the region, reporting nine in 2000 B.C. and eight again in 1800 B.C. plus one in India (for which, however, none were recorded during its Harappan-civilization prime in the 3d millennium).

Some notable shifts in settlement, trade, and perhaps "centers of gravity," if not center-periphery relations,

may also be observed during the 3d millennium. On Harappa's Central Asian frontier, late-3d-millennium settlements appear in Bactria, Margiana, and the Kopet Dagħ piedmont in areas perhaps previously less settled. New evidence shows that, except perhaps in the last-named, urban settlement only shifts location through the late Bronze Age and increases into the Iron Age. According to Kohl (1984, 1987a), this evidence contravenes the previous impression of an urban collapse. He also argues for the probable expulsion of Harappans because of competition for minerals from Afghanistan. Turkmenia, southern Afghanistan including Sistan, and the Indus Valley had had widespread contacts and interdependencies from the end of the 4th to the middle of the 3d millennium, and formal maritime trade linked the Indus Valley and southern Mesopotamia via the Persian Gulf. The Kulli, the closest westerly neighbors of the Indus Valley Harappans, may have played a middleman role in this trade. Moreover, according to Shaikh (1992),

The pre-Indus connections were all overland with Afghanistan, Iran and Turkmenia . . . [and] gave rise to intermediary settlements in Afghanistan and Iran—such as Tepe Yahya, Shahr-i Sokhta and Mundigak. All these three sites lost their importance and came to their last stages of their life when Mohenjo-darians or the Mature Indus people began to forge ahead in their civilizational advance by capturing the world trade market of that time. They now directly approached the mineral source areas. [p. 19]

It was the direct contact with the Gulf countries which led to a new phase of expanding trade and consequent enrichment by . . . Mohenjo-darians as well as . . . the people of the Gulf. [p. 21]

There was a shift in the trade routes from north to south in this period, and greater reliance . . . on *sea routes*. [p. 1]

The coast from Mesopotamia down the Gulf all along the Arabian sea to Gujarat is littered with sites bearing evidence of the Mature Indus period. [p. 3]

The Oman sites not only point to connections with the Indus valley, but they have a marked relationship with southeastern Iran sites. [p. 12]

The rise of the Gulf sites seems to coincide with the rise of Indus cities in the east. [p. 6]

The important thing is that sea connections were evident only in the Mature period, neither before nor after it. [p. 3]

It was because of the Indus-Mesopotamia contacts that there was a rise [of] intermediate sites along the Persian Gulf making them international. [p. 2]

My only doubt would be whether it was the Harappans' direct contact with the Gulf that brought expansion and wealth as Shaikh says or whether it was not general economic expansion that brought about the contact.

In the Gulf region, settlement and economic activity seem to have shifted from the Arabian coast to Bahrain; the evidence here includes the growth of Qala' to some 5,000 and the export of grain staples from Mesopotamia to Dilmun in the closing three centuries of the millennium. Crawford (1991:150) suggests that Gulf states' mercantile rivalry in and for the carrying trade between Sumer and Oman as well as with Meluhha may help explain this shift. As we have seen, for Edens (1992:127–29) population growth, urbanization, and social complexity in Dilmun on the Gulf are probably causally and dialectically related to Mesopotamia, with which it exchanged copper for cereals. These became not a luxury but a staple import into the Gulf during the last centuries of the 3d millennium. The Barbar region is the only one in Ratnagar's account whose high tide persisted into the 2d millennium. The period 2000–1750 B.C., that is, in the next A phase, "was the period of the Dilmun trade par excellence," according to Edens (1992:132). By then, however, the Indus connection seems already to have languished; only a few archaeological finds of Harappan origin in the Gulf region date from this later period (Edens 1990). After that Gulf trade "greatly diminished in volume and the nature of goods exchanged . . . for at least several centuries" (Edens 1992:132).

In conclusion of this review of the 3d millennium B.C., we may ask with Ratnagar to what extent the eclipse of the sea trade may explain the collapse of the Harappan urban system (1981:253):

If the efficient and wide-reaching urban system of the Harappans was generated by trade mechanisms and dominated by a merchant class become powerful by its successful participation in an extensive trade network, and if the "markets" for this mercantile urban system dwindled, . . . the wealth and power of rulers would have been seriously affected. Repercussions of a fall in the quantum of trade could also have been felt by the rural population [if only through the move back to the countryside of newly unemployed urban dwellers. This may be how Harappan civilization] was phased into oblivion.

The dating of the decline and fall of Harappa is still in dispute between the late 3d and the early 2d millennium B.C. Carbon-14 dates suggest an end between 2100 and 2000 B.C. (Ratnagar 1981:206), that is, during the end of what appears to be a more generalized B phase. In that case, perhaps Harappa was "phased into oblivion" also as a consequence of this late-3d-millennium B phase throughout most of the world system in West Asia. "The evolution of the Indus Valley civilization must be explained historically: that is, by reference to those larger processes which all interacting societies of West Asia were experiencing in the latter half of the third millennium B.C." (Kohl 1987b:356).

It would of course be desirable for others better qualified than I to refine the economic cycle and explore its relation to shifts in hegemony and who occupied which musical chairs within the world system in West Asia and elsewhere during the 3d millennium B.C. Such re-

search and analysis might also assuage Kohl's continuing doubts about core-periphery relations. Sherratt and Sherratt (1991a) begin this task for the late 3d millennium but concentrate on the 2d.

2d-Millennium Cycles

The extent and shape of the world system at the beginning of the 2d millennium and its expansion/contraction and hegemonial shifts through the late Bronze Age remain less clear than would be desirable. Kohl (1987a:23) contends that there was no direct contact from one end to the other—indeed, that “there was not a single Bronze Age world system.” A late-3d-millennium gravitational shift to the Gulf region, which continued into the 2d millennium, has already been noted. In Mesopotamia, activity shifted northward and became more decentralized, with many smaller political units, until the rise of Babylon. Then “the central area of the Near East, from the Zagros to the Mediterranean, and from the Gulf to the Taurus and sometimes beyond to the Black Sea, appears to have formed a natural unit . . . and there was a developed network of routes and exchanges within the region. Egypt is conspicuously absent” (Larsen 1987:53). However, there may have been connections with Cyprus and/or the Aegean. Kristiansen (1993a) goes farther: “Regional interaction between empires of productive irrigation agriculture in the Near East, commercial city states in the Mediterranean, nomads to the north, and ploughland agriculture and mineral exploitation in temperate Europe created a rather unique world system from appr. 2,000 B.C. onwards.” He notes an “intensification of connections” and “a regional hierarchy of indirect C/P [core-periphery] relations.” If we follow Chernykh, however, this “unique world system” and the “intensification of connections” extended all the way across Eurasia north of the mountains as well. On the other side, we have seen that with the decline of Harappan civilization the Indus Valley seems to drop out for about a millennium, at least in regular contacts with the west, although there is some evidence of a turn southward and eastward that has been linked with the emergence of trade and migration between the Indian East Coast and Southeast Asia and the Lapita expansion into Melanesia and Polynesia.

Gills's and my (1992) cycle phases began with a B phase from 1700 to 1500/1400 B.C., and this implies a previous A phase, especially if the 3d millennium ended with a B phase. The evidence, however, is ambiguous, and confirmation or disconfirmation of phase datings by recourse to Chandler's city census remains uncertain until much later in the 2d millennium. His census shows nine cities in 2000 B.C. and in 1800 B.C. However, a city is added in India that Wilkinson regards as spurious; it comes at the time of the extinction of the Harappan civilization, which may have continued its decline during these first two centuries of the 2d millennium. The decline of (southern) Mesopotamia is marked by the loss of three of its six cities in the Chandler census, but

cities in Egypt increased from three to five. The A and B phases and their dating that we have suggested are at least not disconfirmed by evidence from Chandler's city census. Before the rather firm 1200 B.C. date for the final crisis of the 2d millennium, however, the city census data neither inspire additional confidence in our dating nor offer much guidance for a definite alternative dating.

A Phase, 2000–1800/1750 B.C. Beginning around 2000 B.C., a region centered around Bactria and Margiana in Central Asia flourished for some 250 years, ending in 1800–1700 B.C. (Hiebert and Lambert-Karlovsky 1992). (According to Kohl, as I have pointed out, the evidence now disputes the thesis of a total collapse of urban settlement there in the next period.) The Gulf trade flourished in the period, and economic activity increased in Cilicia and Cyprus and then also in Crete and the Aegean, where Minoan civilization began developing in close economic and other (core-periphery?) relations with Egypt and the Levant. More of the Mediterranean and its coasts were incorporated into the world system. Larsen (1987) describes a trading system centered on the middleman role of the relatively small Mesopotamian city of Assur, which flourished apparently independently during the 19th century B.C. and then was absorbed into a larger political unit until Hammurabi unified the whole area around Babylon. Harappan civilization, whose decline in the previous B phase has been noted above, may, however, also have hung on longer. Ratnagar (1981:207) considers a possible end not in 2000 but around 1800 B.C. The later date would be during (and might raise some doubt about) this A phase, and so might the 2000–1970 B.C. wars of unification in Egypt and perhaps the Sumerian wars in the Mesopotamian region in the 19th century B.C.

B Phase, 1800/1750–1600/1500 B.C. Chernykh (1992:305) remarks on "the destabilization of . . . ethnocultural and political systems . . . between the eighteenth century B.C. and the sixteenth century, when obvious signs of universal cultural crises and mass migrations can be observed . . . throughout the eastern European steppe and forest-steppe" and in the eastern Mediterranean and on the simultaneous collapse in distant China, followed later in the 16th century by the emergence of the Shang state. "A whole chain of cultures disintegrated and new ones were formed in their place" (p. 190).

There is evidence of decline from the 18th century B.C. elsewhere as well. According to Edens, the Gulf was apparently in a period of decline; maritime trade had virtually ceased by about 1750 B.C. and remained interrupted for several centuries, "marking a period of regional social disruption" (Edens 1992:132). Simultaneous crises of linked hegemonies have also been noted in Gills's and my account, among them the conquests of Anatolia and Mesopotamia by the Hittites and Kassites while the Hurrians and Hyksos overran the Levant and Egypt. This was another of the recurrent (cyclical?) 200-year-long periods of massive migration primarily but not only out of Central Asia that I have noted in my study of the latter (Frank 1992a). This period of simultaneous

disintegration of hegemonies was accompanied by inevitable economic disruptions and the “disappearance . . . of all vestiges of social reform—or experiments—of the Hammurabi era” after his death about 1750 B.C. (Oppenheim 1977:159). Melko and Wilkinson (1992) refer to an “implosion” in Mesopotamia but not until the 16th–15th century B.C. Silver (1985:161) notes the onset of a “Dark Age” decline of urban life, but his later and rather “precise” dating from 1600 to 1347 B.C. is difficult to accept.

Bosworth and Wilkinson both find that Chandler’s city census confirms a B phase during this period, especially in Egypt, which drops from five major cities in 1800 B.C. to three in 1600 B.C., and in India. Increases are registered, however, for Asia Minor and the Aegean toward the end of this phase. The Sherratts (1991a:369–70), however, witness in more westerly regions “an increase of scale and tempo, with the corresponding friction of growth, between 1700 and 1400 B.C. . . . the political consequences of [which] were to create new, expansive power-centres on the edges of the system, which sought to achieve independence and extend their control over the centre.” This increase in scale, which is not uncommon in periods of crisis in the modern world system (Frank 1978a), helped set the stage for the next A phase.

A Phase, 1600/1500–1200 B.C. Chernykh (1992:306) regards the period from the 16th to the 12th century B.C. as one of “stability”: “A whole chain of new metallurgical provinces, stretching from the Pacific to the Atlantic, was formed. The technology of casting thin-walled tools and weapons and the production of tin-bronzes spread explosively through this entire zone.” In the 16th and 15th centuries, from the Dnieper eastward there was a sharp increase in the amount of mining in new copper and tin ore areas in the Urals, Kazakhstan, and the Altai and on both sides of Lake Baikal. “The huge scale of mining in a number of the mines is astonishing” (p. 190). For instance, 2 million metric tons of ores were mined and some 100,000 tons of copper were smelted at just two Kazakh copper-ore deposits (p. 193). In the eastern Ukraine and the northern Donets basin there were “specialized settlements of professional miners, metallurgists and metalworkers” (p. 193). There was a 1,000-km trade route for tin (p. 194), and copper ore was transported 300 km through the mountains (p. 202). “Steppe and forest-steppe peoples had a uniform economy and were very closely connected to one another. Cultures, it appears, were usually not isolated but consisted of open systems: economic, ideological and kin-based interconnections and exchange were not only possible but, very probably, actively encouraged” (p. 194). Chernykh (pp. 256–61) also reports increased interaction between Eurasian and European provinces between the 14th and 12th centuries.

An A phase of the Tumulus culture in North-Central Europe between 1600 but especially 1500 and 1250 B.C. also appears in diagrams by Kristiansen (1993a). In Europe, Kristiansen (1991:30) notes “an expansion phase. Suddenly, within a generation at about 1500 B.C. the

fully fledged chiefdom structure emerged in northern Europe . . . [in] a period of conspicuous wealth [that] lasted, with some ups and downs, from 1500 to 1100 B.C., but, already in the later part, . . . declined." Kristiansen (1993a) also points to expansion from 1500 onward, along with a shift in trade of northern Europe with the Mediterranean area from an eastern axis via the Danube and the Black Sea towards the western Mediterranean and Italy.

For the Sherratts (1991a:370), also, "these two centuries [1400–1200 B.C.] are somewhat arbitrarily separated from the preceding phase, and mark the climax of the palatial trading system and the political frameworks within which it was carried out." Along with Gills and me, they underline the expansion of the Hittites and Assyrians but also a major phase of urbanization in Cyprus, the importance of Rhodes, and a shift from Crete to the Greek mainland. They also remark on the related "intensive diplomatic activity." Similarly, Liverani (1987:67) reports an exceptionally high frequency of treaties in the 15th to 13th century. Kassite Babylonia was, according to Edens, in its phase of greatest prosperity, these centuries marking the longest period of political integration and economic prosperity in its history. New cities were founded and old ones expanded. Babylonia extended its administration over Dilmun and maintained wide-ranging relations with lands to the west in a struggle with Egypt, Hatti, Mitanni, and Assyria for client states in Syro-Palestine. Nonetheless, the Mesopotamian region experienced a long peace from 1380 to 1331 B.C. (Melko and Wilkinson 1992).

Gills and I (1992:637) have observed that

dominant but inter-linking hegemonies were the Hittite empire, based in Anatolia and dominant in northern Mesopotamia, and the empire of New Kingdom Egypt. The period was clearly marked by the prominence of inter-linking hegemonies, including Babylon, Assyria, and Mitanni, all of which took a full part in the well developed diplomatic discourse of the period. There was for a time something like a concert of powers among these inter-linking hegemonies. The Mycenaean trade supplanted the Minoan in the East Mediterranean.

The Sherratts (1991a:372–73) summarize:

This [1400–1200 B.C.] period represents the climax of bulk maritime trade in the Bronze Age. It differs from the 15th-century patterns in the disappearance of Cilicia and Crete as major centres in their own right, and the emergence of Cyprus as a major international trader. . . . The system thus seems to have differentiated into two components: a major long-distance international route marked by port towns and emporia such as Tell abu Hawam, Ugarit, Enkomi, Ialysos, Kommos, and stations like Mersa Matruh, and operated by ships with large cargoes . . . heavily capitalised and partly state dependent; and a series of cycles operated by long-distance ships of smaller capacity in the west, some controlled from

mainland centres but many under less centralised control. . . . Not all this activity, however, should be imagined as the peaceful growth of commerce. Factors of international competition are evident in the insecurity of the Levant revealed by Amarna letters . . . [and] Egypt and the Hittites clashed at Kadesh in 1284.

Thus this A phase, which Gills and I dated only from 1400 to 1200 B.C., probably began at least one and perhaps two centuries earlier. Bosworth interprets Chandler's city data to "lend strong support" to the proposal of this period as an A phase. Wilkinson treats it as a "rally," despite some misfitting data, and notes four political-economic "peaks" in the period 1600–1400 B.C. and six peaks in the succeeding period 1400–1200 B.C. However, from 1360 B.C. to 1200 B.C., the number of major cities declines by one, and there is one less city in the Aegean/Mediterranean, Asia Minor, Egypt, and the Levant. The beginnings of the introduction of iron, especially in weapons but also in tools, initiated the beginnings of a major transition and the final crisis of the Bronze Age.

B Phase, 1200–1000 B.C. Gills and I (1992) have called attention to Childe's (1942:185) belief that "the Bronze Age in the Near East ended round about 1200 B.C. in a dark age. . . . Not in a single state alone but over a large part of the civilized world history itself seems to be interrupted; the written sources dry up, the archaeological documents are poor and hard to date." Liverani (1987:69, 71) comments on "the collapse of Near Eastern Civilization . . . [whose] crisis is rather extended and takes place at roughly the same time over a large area" (p. 69) and observes that the scarcity of surviving documentation "is not fortuitous . . . [but] is itself an effect of the crisis (eclipse of scribal schools and palace administrations)" (p. 71). For instance, 576 years of Kassite domination in Mesopotamia came to an end in 1171 B.C. The Aegean-based Mycenaean civilization came to an end about the same time. Another 200-year-long wave of migration brought Indo-Europeans eastward toward the Tarim Basin and Aramaeans, Dorians, and others into the Levant and Greece (Frank 1992a).

Chernykh echoes this theme when he writes that "these processes of widespread migration and the related collapse of cultural systems reached a peak in the eleventh to tenth centuries B.C. Archaeologically speaking, this is the boundary between the Bronze Age and the Iron Age, and one of the most significant and critical periods in the history of the peoples of the Old World" (1992:306). He notes "a sharp decline in the production of bronze artefacts throughout the Eurasian steppe at the end of the LBA [Late Bronze Age]" (p. 262) and from 1200 to 1000 B.C. a "collapse of the system" in the Irano-Afghan province, where "the settled way of life of the local population apparently changed to mobile pastoralism" (p. 272). There was a similar decline in bronze casting throughout the northeastern Balkans and the Carpathians (p. 262) and a sharp increase in mobile subsistence strategies (p. 243).

Kristiansen (1993a) links the "collapse" of the Mediterranean and Near Eastern regional systems shortly after 1200 to Europe and refers to evidence of mercenaries and later southward migrations from Central Europe and the Balkans. In Europe, agriculture became more dominant and political organization less chiefly, more decentralized, and more "populist," with a more "democratic ideology." James et al. (1991:279) speak of "centuries of darkness" and write that "the term 'Dark Age' seems like an understatement when the archaeological remains from Babylonia . . . are examined." They continue (p. 311):

There can be no doubt that in many parts of the Old World there was a dramatic collapse at the end of the Late Bronze Age. The centralized economies controlled from the palaces disintegrated, the old trading markets broke up, diplomatic contacts were lost and major settlements were abandoned. However, the causes . . . are unclear. . . . Ten separate interpretations of the events at the end of the Late Bronze Age can be discerned . . . [including] cultural decadence [à la Toynbee] . . . climatic catastrophes . . . and invasions by outside barbarians—notably the Sea Peoples. . . . Such "external" causes are rarely convincing because they cannot themselves show why the civilised society was unable to cope.

Kristiansen (1987:84) notes that "with the decline of international exchange networks of prestige goods at the transition to the Iron Age, the whole system of center/periphery relations collapsed. The various regions developed autonomous cultural and economic traditions." Sherratt (n.d.:13) also remarks how "in the final centuries of the second millennium . . . the long-distance north-south links temporarily slackened. The Nordic region developed on its own, without plentiful supplies of metal from further south." Elsewhere, Sherratt and Sherratt (1991a:373–74) point out that

the collapse of large-scale inter-regional trading systems began in the most heavily capitalised areas, and its effects reached outward to involve all the palace economies which were dependent on them. . . . Ugarit . . . was permanently destroyed some time shortly after 1200 BC, along with neighboring Atchana and Carchemish. The recession in Syria which followed these destructions had further effects on the Hittite hinterland, and economic difficulties exacerbated local unrest, leading to the destruction of Bogazköy itself at this time. The Assyrians under Tglath Pileser I (c. 1100) took advantage of this unstable situation to invade Syria and the Levant, before the shift of power to semi-nomadic Aramaean tribes caused a fundamental decentralisation of local economies (and, incidentally, a new set of inland routes made possible by use of the dromedary).

In Babylonia, Kassite decline began with the Assyrian invasion in 1225 but culminated in their ouster in 1157 B.C. Political authority was increasingly decentralized as

peripheral provinces detached themselves from effective state control (Edens 1992).

Gills and I have noted that at the same time the Mycenaeans in Greece and the Levant were overrun by new waves of invaders—Dorians, Aramaeans, and Phoenicians. The Hittite empire disintegrated. Political power almost everywhere was unstable and short-lived. Egypt was invaded by the Sea Peoples. The Mesopotamian region experienced the Aramaean wars beginning in the 11th century, and from the 12th century onwards the Aryan wars raged in India.

Chandler's data on cities also support the B-phase designation according to both Bosworth and Wilkinson. Cities cease to grow, the number of major ones declines a bit, and Hittite and Aegean cities disappear altogether. Wilkinson notes a marked decline of political-economic "peaks" from six between 1400 and 1200 B.C. to only one between 1200 and 1000 B.C. and observes "a more noticeable character of disintegration . . . than in preceding centuries."

These "dark ages," however, are often considered to have lasted up to 350 years and well into the 1st millennium. James et al. (1991) identify inconsistencies and hiatuses in regional datings of the crisis, which sometimes leave 350-year gaps during which on the evidence virtually nothing seems to have happened. Sherratt and Sherratt (1991b) accord James et al. more success in demonstrating the dating problems than in resolving them by their readjustment of the relation between dating sequences in Egypt and elsewhere. However, to the extent that their shortening of the crisis period is well taken, Gills's and my designation of 1000 B.C. as the end of this B phase and the beginning of the next A phase also gains in credibility.

In summary, for the Bronze Age and especially the 2d millennium B.C., there seems to be substantial evidence for a cycle with long phases of expansion in production, trade, and cities, apparently accompanied by more extensive hegemonic rule and greater political stability, and phases of slower growth, contraction, crisis, and even dark ages, marked by economic and urban decline, more massive migrations/invasions, sharpened social and political conflict both "domestically" and "internationally," and accelerated ethnic and cultural diffusion or fusion. In the 2d millennium, this cycle is marked by alternating phases that can be tentatively dated 2000–1800/1750 (A), 1800/1750–1600/1500 (B), 1600/1500–1200 (A), and 1200–1000 (B) B.C. These phases and their manifestations seem remarkably synchronized over an immense area stretching across Afro-Eurasia from Europe and the Mediterranean through West and Central Asia to eastern Siberia. This outline may offer a basis and framework for devoting much greater attention, with regard to the 2d millennium B.C., to questions that have had to be left unanswered: Was there a west-to-east time lag in cyclical displacement? What were the related regional shifts in core-periphery positions and the ups and downs of hegemony and rivalry within this vast world system? Did the system already have a hegemonial center, and if so, where, when, and how did it shift?

Doubt also persists about the extent and timing of the participation of India, Southeast Asia, China, and Manchuria-Korea-Japan in this 2d-millennium Bronze Age world system. It remains less than clear whether the regions in India that were incorporated into the world system in the 3d millennium "dropped out" or only temporarily "involved" in a dark age of their own analogous to what would befall Western Europe two millennia later—or, alternatively, perhaps turned eastward, toward Southeast Asia, in the 2d millennium as in the 1st. Events in the Chinese region seem synchronized for a time with those elsewhere, according to Chernykh, but does this really mean participation this early in the world system?

Exploring the World System in the Early Iron Age

Extending this exploration of the spread of the world system and the identification of its long cycles through the 1st-millennium-B.C. Iron Age is at the moment more problematic (at least for me). Among the academic reasons for this is that for the 1st millennium B.C. Chernykh's review of (northern) Eurasia offers a less detailed guide through its cycles. Other sources are also less systematic and/or complete, particularly regarding the more easterly regions of the world system. Especially for these regions, Chandler's city data as analyzed by Wilkinson and Bosworth are also less complete or reliable, displaying more ambiguity with regard to the identification and/or dating and the regionality of the cycle phases. The real-world reason, probably underlying the academic ones, for these problems is that the world system itself seems to have experienced dramatic expansion and transformation during the 1st millennium. In particular, India became more (re)integrated, and Southeast Asia and China definitively joined the world system. Developments in the east seem to have been more rapid, albeit less well recorded, even while the west of the growing world system was experiencing longer and better-recorded B phases. Although regional rising suns during systemic B phases are not unusual, in this case their large-scale but poor recording in a sort of bifurcation of the world system presents additional difficulties for the exploration of the extent of the system and the dating of its cyclical phases—at least for me and for now. Therefore, but also to avoid lengthening this essay beyond all bounds, I extend it into the 1st millennium B.C. only summarily and briefly.

In general, during this period, economic and political crisis seems to prevail more in the west, while regions to the east may have been laying the basis for more accelerated growth that may foreshadow its approaching inclusion in the central world system. A bird's-eye view of these world-system-extending transformations is as follows:

Beginning with Europe, "it might be suggested that the structural divergences created during the first millennium B.C. between northern Europe, central Europe

and the Mediterranean determined the later course of European history by establishing the structural foundations upon which it came to rest, e.g., the limits of the Roman empire in Europe" (Kristiansen 1993a).

Transformations in West Asia are summarized by Ghirshman (1954):

The first half of the first millennium B.C. was a turning point in human history. The centre of "world politics" or of the age shifted . . . [from alluvial valleys in the south] more to the north . . . the struggle for world power was centered . . . [upon] three principal actors in the drama: the Semitic Assyrians with their vast empire; Urartu, a powerful kingdom of Asiatic origin, tenacious opponents of the Assyrians . . . and finally the Aryans, the Iranians who, after a long and arduous struggle, triumphed over their two adversaries and, with the spoils, founded the first World Empire [under the Achaemenid kings from the 5th century onwards]. [p. 75]

There was a shift in the centre of gravity of exporting countries. Assyria, which was a great consumer, had no iron mines; for a time, especially during the earlier half of the eighth century B.C., it was denied access to the mining centres of the southern coast of the Black Sea and Transcaucasus by the neighboring kingdom of Urartu. Inevitably it turned its attention to Iran [which obtained this metal from regions inaccessible to Assyria]. [p. 88]

Karl Jaspers (1949, 1955, 1957) called this mid-1st-millennium-B.C. period the "axial age," regarding it as the turning point in human history. He also noted, as have Teggart (1939) and McNeill (1963), that the prophets of the great religious movements were born at almost the same time in the 6th century B.C.: Pythagoras in Italy, Thales in Greece, Ezekiel and the second Isaiah in the Levant, Zoroaster in Persia, Buddha and Mahavira in India, and Laozi and Confucius in China. These three scholars and others have suggested or at least implied that this simultaneity was probably no accident. According to McNeill (1963:338), "if the social and psychological circumstances of the submerged people and urban lower classes were in fact approximately similar in all parts of Western Asia, we should expect to find close parallels among the religious movements which arose and flourished in such milieux. This is in fact the case." Indeed, Gills and I have suggested that these similar "social and psychological circumstances" may reflect similar economic circumstances and at least an immediately preceding common economic crisis. The emergence of universalist religions may also be an indication of a high level of real economic linkage and perhaps the attainment of a new level or stage of economic integration, for it is also in this "axial" period that China seems to have become a permanent part of the central world system. The midmillennium saw yet another of the half-millennial-recurring waves of Asian migration, this one later remarked on by Herodotus.

Less well-studied is the apparent incorporation also of

Southeast Asia. Bronze may have been in use there already in the early 2d millennium B.C. However, the Southeast Asia scholar George Coedes (1968:7), following van Stein Callenfels, dates the arrival of bronze in Indochina around 600 B.C. and in the islands around 300 B.C. This dating for bronze may also be very late, because iron finds date from as early as 750 B.C. Archaeological finds also establish significant contacts and trade of tin and gold between the islands and the Malayan Peninsula and mainland from the middle of the 1st millennium B.C. (Raman 1991). Although I have cited references to Indian influence in Southeast Asia from the early 2d millennium, the "recorded" beginning of its "Indianization" is in the mid-1st millennium (Coedes 1968, Glover 1991). Indian texts attest to "speculative mercantile voyages for commercial profit, financed by merchant guilds in many parts of India" in the 4th century B.C. (Glover 1991). At the same time, according to Chinese texts, their merchants traveled and carried silk over the "southwestern route" from Sichuan through Yunnan across Burma into India. This route became prominent again in the 1st centuries B.C. and A.D. Moreover, there was "considerable trade" between Chinese and Yue to the south in China and Indochina before the end of the 3d century B.C. A Qin emperor sent five armies of 500,000 men against the Yue to secure economic spoils after 221 B.C., after which merchants from both sailed at least as far south as Annam. The next expansion of the "Nanhai" trade with Southeast Asia and with India came during the Han Dynasty in the 200/100 B.C.–A.D. 200 A phase (Wang 1958).

The writings of Ptolemy and the famous *Periplus of the Erythrean Sea* attest to regular maritime trade between the Roman empire and the west coast of India. However, trade was equally or more intensive onward from the Coromandel east coast and Ceylon to Southeast Asia and China. For instance, Francis (1989, 1991) has done research on Arikamedu in eastern India and its bead manufactures, which were geared to export both westward to Rome and eastward to much of Southeast Asia. Francis (1991:40) writes that "it is no longer adequate to think of it [Arikamedu] as an 'Indo-Roman trading-station' or to assess its value only in terms of its interaction with the Mediterranean world. The data from other sites [in Sri Lanka, Vietnam, Thailand, and Malaysia and possibly Indonesia] show that Arikamedu looked east far more than it looked west." Chinese Eastern Han Dynasty documentation also attests to significant trade with Southeast Asia in the 2d century A.D., and there is evidence of the same from the 2d century B.C.

Moreover, according to Glover (1991:n.p.)

the great expansion of Southeast Asian and particularly Island-Mainland exchange which is evident in later prehistory is, I believe, closely connected with this Indo-Roman commerce and can be explained in part, at least, by a rising demand. . . . [recent] finds . . . are enough, I believe, to permit us to argue that regular exchange links between India and

Southeast Asia commenced earlier than Wheeler or Rashke allowed. . . . By the early Christian era these trade routes reached out to bring together the previously rather separate Southeast Asian exchange systems, linking them into a vast network stretching from western Europe, via the Mediterranean basin, the Persian Gulf and the Red Sea, to India, Southeast Asia and China . . . [in] what has been called the World System.

Gills's and my (1992) cycle phases for this period are rather uneven in length, and their datings are uncertain. Moreover, the B phase from 800 to 550 B.C. is not reflected in Chandler's city-size census. This is partly, as Bosworth suggests, because these and later datings are excessively influenced by events in West and Central Asia and neglect possible more important developments farther east. Therefore, it may be well briefly to review these datings without, however, attempting to advance very far beyond them.

A Phase, 1000–800 B.C. Sherratt and Sherratt (1991a:375) remark that "the system was revitalised . . . in the 10th century," particularly along the spice route from Arabia and by Levantine-centered trade of "pan-Mediterranean scope," and Sherratt and Sherratt (1993) extend this analysis. Kristiansen (1982) refers to Phoenician expansion through the Atlantic to France and Britain in the 9th and 8th centuries B.C. Gills and I (1992) have similarly underlined the Phoenician expansion through the Mediterranean during this period and noted the rise of and then the challenge to Assyrian power on the mainland. After 1000 B.C., however, metal supplies also increased again in distant England and Scandinavia (Kristiansen 1993a). Indeed, the increase was of such enormous proportions in the west in its final phase as to suggest "overproduction" and the use of Armorican axes as currency. However, "it can hardly be doubted that large-scale metal consumption and inflation in the west was somehow related to the decrease of metal production in the east. . . . A new axis of exchange emerged (during Ha[llstatt] B2-3), stretching from northern Italy over Switzerland to the Lower Elbe and further on to Scandinavia, Northern Germany, and Pomerania." Dietler (1988:129) argues for the important intermediary role of the inhabitants of the Rhone Valley in articulating and perhaps even initiating such long-distance north-south trade and fostering "dependent relations of a center-periphery nature." Wilkinson finds this phase confirmed by the city data. Bosworth agrees and finds corroboration for Assyria but suggests that "perhaps this A phase might be extended as Nineveh, the seat of Assyrian power, peaks somewhere between 800–650 B.C., when it reaches 120,000 people—the first city on Chandler's list to break the 100,000 mark." Indications by the Sherratts (1993) could also extend this phase into the 8th century. Continued growth is evident especially in Assyria, and there are new developments in northern Mediterranean regions in the Aegean, Villanova Italy, and Spain. The number of major cities from the Mediterranean to India remains the same at ten, however, and

increases from three to five in China. Melko and Wilkinson (1992) record both peace (810–745 B.C.) and war (859–810 B.C.) in West Asia and war in the 8th and 7th centuries in South Asia.

Edens (personal communication) finds this phase politically heterogeneous and tends to doubt that it was an A phase. He observes that Babylonia continued to collapse, but we need not regard this as contrary evidence. The Assyrian empire flourished only in the 9th century, but this again is not disconfirmatory. Egypt was parochial, and multiple states were in competition in Syro-Palestine. The South Arabian spice trade is “over-rated.” Admittedly, persuasive evidence is sparse, even though there seems to have been an urban revival in India and some integration in China under the Western Zhou Dynasty in the 10th century.

*B Phase, 800–550 B.C.*² Identification and dating of the next phase are particularly problematic, and Edens regards it as “heterogeneous.” Chernykh’s coverage of Eurasia is less revealing for this period. Gills and I (1992) have noted increased competitive pressures in the Mediterranean and rivalries in West Asia, as well as the presumably related collapse of the Assyrian empire in the 7th century B.C., but also technological/economic development in India and new rivalries in China. The Sherratts (1993:369–74) point to a “great bifurcation” in the 7th century, in which local production replaces Phoenician trade in the Aegean, and a “growing differentiation” in the Mediterranean in the 6th century. Then, however, “the rapid growth of the Median and the Persian Empires brought a new scale of integration from the east Aegean to the Indus” (Sherratt and Sherratt 1993:371).

Wilkinson and Bosworth single this phase out among Bronze and Iron Age ones as the most challenged by the Chandler data on cities. Wilkinson says that it is not reflected in Chandler’s data; Bosworth does find an apparent “period of ‘contraction’ and ‘fragmentation’ but only in the western part of the Old World. Between 800 and 600 BC, there is little growth for Babylon, Jerusalem, or Van, and other cities drop from the list entirely. By contrast, Chinese cities roughly double in size, and another Indian city appears.” Melko and Wilkinson (1992) record wars ranging from Egypt and Mesopotamia to South Asia but peace in East Asia during this period.

*A Phase, 600/500–450/400 B.C.*³ For the next phase, Gills and I (1992) have noted the economic development in Greece, replacing that of the Phoenicians, and especially in Persia. This period witnessed the rise of the Achaemenid Persian empire, which stabilized much of West Asia by reimposing a more unified political order in that part of the world system. The Achaemenids from Darius to Xerxes achieved at least a regional position of hegemonic accumulation in the world system on the basis of the imperial tribute system. The Persian empire exceeded even the Assyrian in the degree to which it incorporated the most important economic zones of the world system in West Asia. There was at this time a shift in the center of gravity of the world economy of very great historical importance; the key area of logistical linkage in the world economy/system shifted from

Syria and the Levant to Central Eurasia. Achaemenid control of Central Asian cities such as the great city of Bactra and the northwestern Indian trading center of Taxila was very important in consolidating Persian hegemony and accumulation. The Persian investment in infrastructure included the 1,677-mile Royal Road that Darius built from Ephesus to Susa and the road from Babylon to Ortospana (near Kabul). Persian cities, like their Assyrian predecessors, were cosmopolitan, and Persian armies were multinational. It was in this period, according to Franck and Brownstone (1986:65), that the great caravan cities of Syria—Aleppo, Hama, Homs (Emesa), and Damascus, in particular—truly came into their own, receiving goods from the Silk Road as well as spices and perfumes from Arabia's Incense Road and other luxuries brought by sea from India. Aramaeans . . . were such active traders in these caravan cities that their speech became the common commercial language."

Edens and Bawden (1988) offer a "case study" of the continuity of occupation but also the ups and downs of a single small locality, Tayma, in the Arabian Peninsula. Its settlement history reflects the geographical, temporal, and product movements in the interregional exchanges on which it depended throughout the 1st millennium. Their findings suggest (pp. 75–76)

that the largest population, most extensive settlement and most intensive activity in the basin occurred during the middle centuries of the millennium . . . [in] the 6th–5th centuries . . . [which] is exactly the period of most intense interaction with Babylonia. . . . The periods before and after this mid-1st millennium flourishing present contrasting patterns of rise and collapse of settlement . . . [and] rapid economic and political disintegration of the city.

In the west, Kristiansen [1982] focuses on important events during Hallstatt D, between about 600 and 450 B.C., in Europe. Central Europe and the Balkans were reintegrated or more fully integrated into the Mediterranean and it in turn into the West Asian world (system?). Thus, however, northern Europe was marginalized (Kristiansen 1982), and the Rhone-corridor trade to Hallstatt Europe broke down again in the early 5th century (Dietler 1989). In Central Europe, the Hallstatt cultures that had first "climaxed" then "declined," according to Kristiansen, as trade routes again shifted and/or they overexploited their peripheries.

Since this phase falls within a longer period between two of Chandler's city censuses (for 650 and 450 B.C.), Wilkinson observes that the data are ambiguous but "at least not out of sync." Bosworth sees them as broadly confirmatory but suggests that "Frank and Gills' focus on Central Asia as the locus of this A phase seems to be misplaced, as events there are eclipsed once again by those farther east" in China and Korea. Bactra and Taxila do not appear on Chandler's list for this period. I might retort that commercial importance is not necessarily always reflected in population size (consider, for

example, Hong Kong today), but Bosworth notes that "China begins as practically a footnote in Chandler's list and by 430 B.C. it has seven of the world's largest 25 cities and the second largest. . . . This dramatic rise cannot be over-emphasized." Whether Gills and I are guilty of a "western" (that is, Central Asian) bias at the expense of China is worthy of consideration, but the "dramatic rise" of China suggests its growing commercialization and probably relations with and incorporation into the central world system at this time. Melko and Wilkinson (1992) record a long peace till the mid-5th century in West and East Asia but wars in the west and south after 550 B.C.

B Phase, 450–350 B.C.? Although Gills and I have inveighed against excessively Greco-centered readings of the subsequent phase, we have identified this relatively short B phase largely on the basis of symptoms of economic crisis in Greece and its relations with Persia. Intensified class struggle and wars seemed symptomatic of an underlying economic contraction or slowdown (de Ste. Croix 1981). Rostovtzeff (1941) characterizes the 4th century as one marked by increased proletarianization, landlessness, unemployment, and food shortage, by a contraction in the market for manufacturers and the ruin of "free" petty producers, and by an overconcentration of wealth in the hands of the commercial and landed ruling classes. Livy notes a series of famines in Italy in 490, 477, 456, 453, 440, and 392 B.C. The Celts invaded Italy and sacked Rome while setting up the kingdom of Galatia in Asia Minor. The hegemonic disintegration of this period is evident from the Peloponnesian wars, the successful revolt of Egypt against Persia ca. 400 B.C., and the breakaway of the Indus from the Persian empire ca. 380 B.C. The phase is too short to be well reflected by Chandler's data, with a longer time span between city censuses. Nonetheless, Bosworth notes that the size of Athens declined but that of Rome increased severalfold.

Kristiansen (1993a) remarks that "the apparent correlation between competitive changes in Greek, Phoenician and Etruscan trade routes with the geographical movement and collapse of princely centers [in North-Central Europe] has been seen as a confirmation of the dependence on long distance trade and the supply of prestige goods." Chernykh (1992:306) terms this "the next 'destructive' period," when Celts moved from Western European toward the Balkans and Asia Minor in the 5th century and Sarmatians in the opposite direction in the 4th. He notes the political destabilization of the Warring States period in China and new confrontations with Central Asian pastoralists. "The high-water mark of these destructive processes and the disintegration and re-formation of cultures in Eurasia was the fourth to third centuries B.C."

A Phase, 350–250/200 B.C.? Gills's and my identification and dating of the following phase rests primarily on the Alexandrian expansion through West Asia into Central Asia and India and the economic expansion in India under the Mauryas in the 3d century after Alexan-

der's death and failure. At the same time, the Qin dynasty consolidated its rule in China, and trade increased between these regions. Here again, Chandler's census dates are not very helpful. Bosworth notes the prominence of Alexandrian cities, including Alexandria itself as the third largest in the world, during this phase, but he suggests that, again, the east may deserve top billing: In 200 B.C., for the first time the world's largest city is in China (Chang'an, with 400,000 inhabitants) and the second largest is in India (Patna, with 350,000). China and India now also have similar large shares of urban population in the world's top 25 cities.

B Phase, 250/200–100/50 B.C. Gills and I have noted another brief B phase in the Mediterranean region, including Egypt and Greece, during the 2d century B.C. In Egypt, the 2d century appeared characterized by all the signs of economic decline, such as overtaxation, official corruption, increased debt, and unrest and brigandage. The Rosetta stone characterizes the period in terms of "pressure of taxes, rapid accumulation of arrears and concomitant confiscations, prisons full of criminals and debtors, public and private, many fugitives scattered all over the country and living by robbery, compulsion applied in every sphere of life." (Edens [personal communication] objects to this interpretation of crisis in Ptolemaic Egypt.) There were also signs of crisis and slave revolts in Rome, but there was also imperialist expansion westward that presaged imperial Rome. While this phase is again too short to be reflected in Chandler's census dates, which jump from 100 B.C. to A.D. 200, Bosworth writes that "there is consensus that this was a period of 'contraction' and 'decline.'" However, China had already begun its period of expansion under the Western Han Dynasty after 200 B.C. Since imperial Rome was also already expanding, perhaps the beginning of the next A phase should be moved forward a century or so. This would convert this B phase into a still shorter and more localized phenomenon, hardly worthy of the name.

A Phase, 200/100 B.C.–A.D. 200. The high-water mark of the expansion phases Gills and I have identified saw the simultaneous rise to imperial grandeur of Han China, Kushan India, Parthian Iran, Axium in East Africa, and imperial Rome. Their rise was followed by their simultaneous decline in the major B phase from A.D. 200 to 500, which was again accompanied by another major wave of invasions, including that of the legendary Attila the Hun. McNeill (1976:96–114) observes demographic expansion during the period of this A phase and epidemic disease-related population decline in the following B phase at both the Chinese and the Roman end of Eurasia. Roman writers such as Pliny and more recent ones such as Teggart have argued that what happened at one end of this chain substantially affected what happened at the other. In this regard, writing from another perspective, Gernet (1982:19) notes:

Just as the power of the great nomad empire of the Hsiung-nu in the steppe zone was probably created

and strengthened by import of iron and silks from China, Han expansion in Asia was certainly due fundamentally to the economic upsurge of the Chinese world. Not only were Han China's strength and prestige abroad based on this economic prosperity, but . . . also the trade with Mongolia, Korea, central Asia, South China, and northern India. . . .

Agreed, except for the (also Western) Sinocentric perspective that sees the "economic upsurge" in all these regions and even the power of the Xiongnu in Central Asia as propelled fundamentally by "the Chinese world." Apparently the economic upsurge was not confined or due fundamentally to China, or else it would not have so easily included all the other areas Gernet mentions, not to mention many more across Eurasia.

Unfortunately, Chandler's census dates again do not match the suggested cycle dates, and this makes the fit problematic. Nonetheless, Wilkinson and Bosworth also accept this period as a major A phase. Maybe its beginning should be dated up to a century earlier in China or even Rome and its end perhaps a century earlier in Kushan India. Writing of course quite independently, Chernykh (1992:306–7) also remarks that "the period of renewed stability, which lasted from the second century B.C. to the second/third century A.D., was related to the existence of three major empires: the Han Empire on the eastern flank of the Eurasian landmass; Rome on the western flank; and the Parthian and Kushan kingdoms in the centre"—before they all again succumbed to the "truly colossal destructive processes and migrations" which were "linked with the destruction of this system." These processes fall in the next major B phase, from A.D. 200 to 500, and are beyond the scope of this paper (but see Gills and Frank 1992, Frank and Gills 1992–93).

Conclusion

This essay has sought if not yet to collect more data at least to begin systematizing some additional relevant data which are now available to me as a nonprofessional on short order. It would of course be desirable for professionals more qualified than I to assemble this puzzle more competently and fully, giving much more weight than I have to migrations, invasions, and wars. In the meantime, and on the basis of the evidence summarily presented above, I suggest that substantial archaeological evidence and important analyses thereof seem to confirm the existence of long cycles in what may also for this reason be called an at least 5,000-year-old world system. Alternating expansive and contractive phases reach back through the 3d millennium B.C. and probably into still earlier times. They are synchronic over so large and growing a part of the world that it truly appears a world system. The question arises whether the socio-political-economic mechanism that generates this "cycle" is also at least partly endogenous to this system and the part of the world that it includes.

Interestingly, increases and declines in city sizes, as recorded by Chandler and reproduced by Wilkinson, also characterize the “Western” hemispheric regions of the “New” World in the “Americas” before Columbus “discovered” them 500 years ago. However, their phases and cycles are totally *unsynchronized* with the fluctuations in city size and/or the phases of expansion and contraction suggested and dated in the “Old” World and refined above. This implies that the substantial confirmation and occasional adjustment of cycle phases by city sizes in Eurasia is not fortuitous (Wilkinson [1992] calls it a fit too good to disregard) and does reflect some underlying reality. If that underlying reality is climatic change, which is only *common* to all parts of the “system” and to which they independently react in tandem without significant interaction with each other, should we then still call them part of a single system? Perhaps an ecological system, but not a social one? I hope that I have marshaled sufficient evidence to show that different regions and peoples have also been so linked through economic, political, migratory, sociocultural, and other both cooperative and conflictive relations as to meet the criterion of systemic participation in a single world system. The participation of the parts is so interactive that no part of this system would be as it is or was if other parts were not as they are or were—albeit they may all have also reacted to, and on, global ecological constraints.

Notably, major periods of near world-systemwide migration accompanied several of the B phases. The significance of this “coincidence” remains unclear. However, Chernykh notes that

these are critical periods in human history. The migration at the end of the second and the beginning of the first millennium B.C. defined the boundary between the Bronze Age and the Iron Age; the later migration [in the A.D. 200–500 B phase] defined the boundary between antiquity and the mediaeval period or, in Western historiographic terminology, feudalism. [p. 303]

We are going into the unknown when we try to understand the reasons for such explosions and successions . . . to identify the hidden driving force behind such phenomena. We suspect that such explosions follow some regular rhythm; that, in accordance with this rhythm, various provinces at the same time either collapse or emerge; however, the nature of this rhythm is equally unclear. [p. 296]

However, the “external” and “internal” problems need not have been unconnected, let alone mutually exclusive. Invaders were more likely to succeed when their target was already economically and politically/militarily weakened by its own and regional or systemwide crisis. Moreover, the invasions themselves were often generated by survival problems in their own areas of origin and/or other pressures from beyond them, partic-

ularly in Central Asia. These considerations, of course, raise largely unanswered questions both about common ecological/demographic changes or cycles and the extent of the "world system" into and the "centrality" of Central Asia (Frank 1992a). In a similar vein, Chernykh also argues, with regard to the connection between large-scale movements of people and the collapse of systems, that "displacement by another group is by no means always the root cause of the formal alteration in and destruction of a culture. Much more often, perhaps, movements of populations are brought about by reason of the internal conditions of a society, and an increase in deep and hidden processes that require changes in a number of social structures. Another cause is related to ecological changes" (p. 302).

A similar position is espoused by Goldstone (1991), writing, however, about early modern history. He notes recurrent waves of state breakdown and prior population increases across all of Eurasia for which it is impossible to account in terms of local conditions or particular cultural patterns alone. The "structural/demographic model" he offers to explain complicated recurrent interactions is as follows: Demographic pressures derived from temporarily falling death rates impinge on overstretched resources and lead to bureaucratic paralysis. This in turn is derived from elite infighting and large-scale social rebellion, which lead to state breakdown. In Goldstone's "model," all of these events are thus ultimately—but socially and not simply physically—generated by climatic change. Perhaps, *mutatis mutandis*, his approach may prove fruitful in the analysis of crises involving political-economic institutions in earlier periods as well.

A possible complementary explanation is suggested by the historian McNeill (1976). In addition to the observations on population increase and decline for the 100 B.C.–A.D. 200 and A.D. 200–500 A and B phases respectively, he offers some epidemic datings for earlier periods elsewhere. In his analysis, demographic change and epidemics emerge from the interaction of social structural with biological, ecological, or climatic factors (e.g., "the plagues of Egypt, in short, may have been connected with the power of Pharaoh in ways the ancient Hebrews never thought of and modern historians have never considered" [p. 40]), but the possible relation between recurrent epidemics in an ever more common Eurasian disease pool and the cycles of expansion and contraction observed above should be considered. Such attention to the social consequences of ecology, disease, climatic change, and even their possible cycles is certainly not new. The Old Testament refers to Noah's flood and Moses's parting of the Red Sea, not to mention numerous plagues which may have been promoted or facilitated by climatic change. Aristotle (*Meteorologica* 1:14, quoted by Harding 1982:1) observed:

The same parts of the earth are not always moist or dry, but change their character according to the appearance or failure of rivers. So also mainland and sea change places. . . . Cold and heat increase owing

to the sun's course, and because of them the different parts of the earth acquire different potentialities. . . . This process must, however, be supposed to take place in an orderly cycle. . . . But these changes escape our observation because the whole natural process of the earth's growth takes place by slow degrees and over periods of time which are vast compared to the length of our life, and whole peoples are destroyed and perish before they can record the process from beginning to end. . . . This has happened in Egypt . . . and Mycenae.

Major social breakdown has been related to climatic and/or ecological changes at other times and places in this cyclical review. Among these changes are the drought and desertification after 3000 B.C., the decline of the Indus Valley civilization, attributed to drought, the Dark Ages after 1200 B.C. during global cooling not only at Mycenae (as observed by Aristotle) but across the whole world system, and the same again during the 2nd-to-5th-century declines of China, India, Persia, and Rome and the subsequent Dark Ages in Western Europe (see e.g., Harding 1982, Lamb 1982, Raikes 1984 [1967]). Lamb tentatively also relates the birth of Confucianism and Buddhism during the "axial age" to "great climatic stress" (p. 146). Similarly, expansion of population and settlement may also have been promoted or permitted by more benign climates, such as during the A phase at the time of Christ. Lamb (1984:234) remarks that

students of history can hardly fail to be struck by the apparent coincidence of the high points of cultural achievement in the late Stone Age and Bronze Age development of trade and communication across Europe and of sea-going communications all along the Atlantic coasts and island chains, and again later in Roman times, and thirdly in the high Middle Ages, with the crests of the temperature curve. And the parallelism seems to go into closer detail too.

However, some contributors in Harding (1982) also observe divergent and even opposite cultural developments at the same (climatic and historical) times in northern and southern Europe. Moreover, Raikes (1984 [1967]) challenges established belief in recurrent "changes of climate" (and a fortiori their cycles and/or worldwide effects) and their supposed social consequences during prehistory in general and for the Indus civilization in particular. He rejects all determinism and stresses the human capacity to confront climatic change. However, even Harding and Lamb leave room for human action, including its environmental effect on climates.

Thus, archaeologists and a macrohistorical sociologist and others refer to possible climatic constraints and dynamics. Yet they also insist on *social structural* if not always world-systemic causes and explanations. "There have been several attempts to link oscillations in historical development in particular regions with various natural phenomena, which, in the final analysis, are related

to periodic changes in global climate" (Chernykh 1992:307). Hypotheses of this kind are somewhat questionable, Chernykh writes, because the same kinds of oscillations and connections are not also observable in Stone Age cultures even though they experienced the same climatic changes at the same time. Although elsewhere (Frank 1992a) I have also emphasized the possible role of climatic cycles, particularly in Central Asia, I might here add that the problem Chernykh mentions also works the other way around: The same climatic change can affect different regions differently. For instance, the same period of warming can affect agricultural possibilities in arid and humid and/or high- and low-lying areas in exactly opposite ways. Yet we have found substantial evidence for very generalized simultaneous economic up and down swings over geographically very different areas. Moreover, these economic cycles seem to have become progressively shorter over the millennia (or has our identification of them just become finer?). All the more so, therefore, "the reasons for the changes in the rhythm during the Metal Era should probably be sought principally among the new socioeconomic conditions of the period," including in particular their interactive participation in a single but cyclically developing "world system" (Chernykh 1992:307–8).

In conclusion, we may join Kristiansen in posing the main question about what any and all this means for our world system and its social structural influences or determinants (1993a):

Were all these regional trends and dramatic historical events somehow interlinked? Were Europe, Asia and the Mediterranean so interdependent that major changes in one region would lead to predictable changes in the other regions, forming a kind of interrelated world system? . . . But how did these changes [in Northern Europe] relate to changes in Central Europe and the Mediterranean? Were they somehow connected? This is, in the last instance, dependent on our chronologies, where I do not feel able to decide (at least not at the present moment) if there is a significant time gap or not between these regional changes.

In any case,

No single factor may account for the observed structural changes, although some factors, such as *climatic change*, may reveal striking patterns of parallelism with changes in settlement structure. . . . Climate thus represented both potential and constraints to subsistence, but social and economic forces remain the prime movers when the environment is exploited not only close to, but often beyond its carrying capacity. . . . In such situations climatic fluctuations may trigger the collapse of an unstable economy. [That is] one of the lessons we may learn from Bronze Age sequences.

It is a lesson that seems particularly pertinent to our own times in the year of the Earth Summit.

Comments

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Although I have some quibbles about Frank's use of Near Eastern data, to engage in such a debate would be to miss the point of his important contribution. Thus, I shall limit my comments to the substance of the paper—the contention that a single world system with simultaneous cyclical phases of expansion and contraction has existed for the past 5,000 years or so.

Archaeologists studying the rise and growth of early civilizations have generally failed to give sufficient consideration to the cross-cultural background of social, political, and economic interactions against which such civilizations developed. Perhaps this is explainable in part by the nature of the data available to prehistorians, which are typically limited in geographic scope. To some degree, however, the failure is also conceptual, since social systems neither exist nor evolve in isolation. This is the central thrust of Frank's article, and he is to be commended for reminding us in no uncertain terms that ancient societies, like modern ones, must be analyzed within the context of a dynamic structure of relationships of interdependency, principally (but not solely) economic in nature, that commonly transcend any particular region or social group.

Thus, I fully agree with Frank's forceful restatement of Schneider's (1977) contention that the world-system paradigm developed by Wallerstein (1974) and his followers for the explanation of phenomena connected with the expansion of Europe and the growth of capitalism in the modern world is also applicable to the study of ancient civilizations. Where I disagree with Frank is in seeing a single world system evolving for the past 5,000 years out of an original Near Eastern/Central Asian core with simultaneous cyclical phases of expansion and contraction. For Frank, this conclusion follows from his minimalist definition as to what constitutes a "world system." He sees any empirical evidence of the cross-cultural transfer or exchange of surplus between polities, even if not direct, as a sufficient indicator of an overarching systemic relationship. Thus, if polity A engages in trade with polity B and polity B, in turn, participates in exchange with polity C, then polities A and C, though never in direct contact, are part of a single interaction system profoundly affecting all the polities involved. In my opinion, this formulation takes us back in some respects to the now-discredited diffusionism that was prevalent in archaeological circles in the late 19th and early 20th centuries. Moreover, it misses the point that what affects the historical development of polities A and C is not their participation in a vaguely defined, ever-expanding "system" but rather their direct relationship with polity B. For instance, according to Frank's formulation, in the second quarter of the 3d mil-

lennium B.C. the city-states of Early Dynastic Sumer, the kaleidoscope of contemporaneous city-states of Syria-Palestine, Old Kingdom Egypt, and Harappan centers along the Indus River would have formed part of a single overarching world system. Yet, though individually some of these polities participated in profoundly disequilibratory contacts, both commercial and political, there was no sustained interaction among them *as a group*, and any attempt to link the economic fortunes of Old Kingdom Egypt and Harappan cities in the Indus Valley strains credulity. I would suggest that a systemic connection can be postulated only where it can be demonstrated that direct and regular contacts existed and the societies engaged in them were thereby transformed.

A more appropriate metaphor for conceptualizing the development of ancient societies, I believe, is Kohl's (1987a) suggestion of multiple and partially overlapping world systems, each composed, in turn, of individual core groups exploiting communities within their own culturally and geographically defined hinterlands and each interacting with immediately contiguous systems. One reason that Frank's all-encompassing world system in antiquity is implausible is his failure to consider that ancient transportation technologies imposed restrictions on the ability of ancient core societies to project power over long distances in a timely and cost-efficient fashion. This means that regular institutionalized contacts between different cultures would necessarily have been restricted in geographic extent and that premodern world systems would inevitably have been less well integrated and more fragile than modern examples. This partly explains why prehistoric and early historic world systems commonly collapsed or declined within a relatively short span of time, often well before the pernicious realities of long-term unequal exchange asserted themselves in affected peripheries (Algaze 1993).

In short, Frank presents a persuasive case for the heuristic value of the world-systems model for the study of ancient civilizations. However, his model must be modified to accommodate the fact, already noted by Kohl (1989), that ancient world systems were qualitatively different from their putative modern counterpart.

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I very much like this global and dynamic description of socioeconomic evolution. I think that Frank is right in seeing *geographical interaction* and *interdependency* among various regions of the world as among the factors responsible for socioeconomic dynamics. I tend to see cycles as successive stages of a single (very complex) system. The goal of analysis will be the detection of differences due to geographical-interaction or interdependency variables, but these variables—being, as Frank points out, the effect of a combination of socioeconomic

mechanisms—are not independent. Therefore I prefer to consider his theory a representation system or model rather than an empirical law. A model does not exist in any empirical sense, but there is some heuristic relationship (i.e., analogy) between it and the modeled world.

Frank uses neither modern references nor anthropological research in his discussion of the “ancient economy” debate (see, e.g., Gregory 1982, Appadurai 1986, Miller 1987, McCracken 1988, Humphrey and Hugh-Jones 1992), and I think that to speak of “accumulation of capital” in this context is inappropriate when all we have is some evidence of prestige items. We can, however, consider formation processes in terms of modern economics, despite the qualitative differences between capitalist consumption mechanisms and precapitalist ones. This allows us some “familiarity” with past economic processes without equating them with modern ones. Thus the cycles detected by Frank cannot be confused with Kondratieff cycles. They may be related to Braudel’s (1966) “cycles de longue et courte durée,” which are more dependent on geographical interaction than on laws of the market. At the same time, the current economic crisis is more a Kondratieff cycle than a collapse of the world system (we are living in an A phase, according to the size of the world system and the volume of circulation).

Except for the absence of “capitalist capital accumulation,” however, Frank’s model is perfectly acceptable. Geographical-interaction and interdependency links are the major causes of the ups and downs of precapitalist economies because they directly affect the circulation and consumption of prestige items. Social relations are defined in terms of prestige items, and therefore any change in their value has important secondary effects on social relationships (especially domination and hegemony). Other important variables in the dynamics of precapitalist societies are related to the *structure* of the world system—that is, not only its geographical extent (the number of linked communities) but also the intensity of interdependency among them. Assessment of this latter by archaeological methods is not easy (see Francfort 1992). Because Frank’s paper lacks any discussion of these measures, he cannot compare cycles for their differential potential for introducing qualitative transformations into the system. Some A phases may be more similar to some B phases than to other A phases. It makes no sense to talk about expansion and contraction in absolute terms.

Although the reconstruction of cycles is acceptable in general terms, I have some concerns about the period between 1200 and 550 B.C. First, Frank seems to give too much importance to migrations; they seem to me poor evidence for the explanation of social processes. Furthermore, there is no archaeological evidence of a Dorian invasion in Greece (although there is a context of “collapse” in some centers outside Attica and Euboea), and it is a serious mistake to consider the Phoenicians “foreigners.” Phoenicians were present in Syria in the early Bronze Age or even earlier; they are the “people of Canaan.” Again, as for the A phase 1000–800 B.C.

and the B phase 800–500 B.C., Greek and Phoenician expansion begin in the 8th century B.C. (Aubet 1992, Ridway 1992); therefore 800–500 B.C. must be considered an A phase, because the world system is in expansion (Italy, North Africa, and the Iberian Peninsula are being integrated into the system as new peripheries). Economic life in the east is not, however, really brilliant at this time (Greek migrations to the west point to some crisis in available resources). Again, the quantity of imported material in Cyprus, Euboea, and Tyre during the previous phase (1000–800 B.C.) is so limited that this can hardly be considered an expansion phase. It is, however, somewhat greater than in the period 1200–1000 B.C., and therefore this might be described as a B+ phase. This is an example of the necessity for measures of intensity.

This is a very stimulating paper. I agree with Frank on the global nature and long duration of the world system in which we live and work. As a historian, I am convinced that present social phenomena are a consequence of past dynamics. However, the global system has experienced very important qualitative and quantitative modifications that can be explained in terms of world-system structure and operation. Whereas these changes do not prevent us from comparing the present with the past, they do demand a different (perhaps mathematical?) approach.

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Frank's stimulating essay challenges historians, philologists, and archaeologists to look again at the regions they study. Frank has always been a master at painting with broad strokes, and his latest canvas is yet another brilliant portrait of social reality that will have the critics buzzing for years. As a fellow trespasser on the territories of other disciplines I am sympathetic with Frank's vulnerability to those whose academic lives have been spent focusing on the details of a single locale or time period. My comment will focus not on the particularities but on the analytic aspects of his work.

Among the scholars now applying the world-systems perspective to periods before the 16th century A.D., Frank is a member of the "continuationist" school (for a review see Chase-Dunn 1992). These scholars stress the continuities they see between the modern world system and the earlier regional world system that emerged out of West Asia in the 4th millennium B.C. Though Frank and his colleague, Barry Gills, began by asserting that there had been a single world system for 5,000 years, Frank now admits that "there probably were several such 'systems' in the Bronze Age and later times." He is here focusing on one of these, which, combining his terminology with that of David Wilkinson, he calls the "central world system."

Wilkinson (1992*b*) has mapped the spatial boundaries of both political/military interaction networks and

trade networks (which he calls “oikumenes”) for the expanding network that Frank now calls the central world system. A point which is never made clear in Frank’s discussion but is central in Wilkinson’s is that political/military networks are typically smaller than trade networks. Although Mesopotamia and Egypt had been part of the same trade network since at least 2500 B.C. (and perhaps much earlier), they were not linked into the same political/military interaction net until 1500 B.C. Attention to the spatial dimensions of different kinds of networks might help Frank with the problem of unevenness he encounters in his periodization of the central system into growth and stagnation phases.

I am an advocate of a different subschool—the comparative-world-systemists (Chase-Dunn and Hall 1991). We acknowledge that there may have been important continuities within the central system (which became the global system), and we also see similarities between different state-based world systems such as those that emerged in Mesoamerica and Peru. But we are interested in systemic differences as well as similarities. We contend that even stateless, classless intersocietal networks can fruitfully be analyzed as world systems (e.g., Chase-Dunn, Clewett, and Sundahl 1992), though these were different in systemically important ways. In this we follow the work of Friedman and Rowlands (1977). We also contend that the central system has undergone important transformations of its systemic logic of development. We agree that markets and capitalism have existed for millennia, but we contend that they began to play a dominant role in the developmental logic of the core region of the central system only with the rise of European hegemony. We consider the distinction between capital accumulation and capitalist accumulation a useful one for understanding the differences between the modern world system and the central system before the 16th century A.D.

The alternation of hegemony and hegemonic rivalry in Frank’s scheme underwent a fundamental transformation when core states became capitalist states. It was then that the rise and fall of hegemonic core powers replaced the rise and fall of corewide empires. This is the sense in which Wallerstein’s distinction between “world-empires” and “world-economies” continues to be valuable.

It is not clear whether Frank’s long phases of expansion and contraction are phases of hegemony, as in Modelski and Thompson’s (1988) “long leadership cycle” scheme, or phases of economic growth and contraction or both. A recent study of urban growth and city-size distributions (Chase-Dunn and Willard 1993) indicates that growth and the concentration of power do not always occur simultaneously and finds only mixed support for Frank’s periodization. We do, however, find an interesting simultaneity of these two macrostructural variables when we compare the Mediterranean–West Asian political/military network and the Far Eastern system. Both the pattern of urban growth and a measure of the steepness of the city-size hierarchies were closely correlated between 430 B.C. and A.D. 1500. These pat-

terns of growth and decline do not resolve into neat 250-year rises and falls, but the striking simultaneities of changes at the two ends of the Eurasian continent indicate that something was going on. It is also interesting that India does not demonstrate this kind of simultaneity. I am partial to Frank's theorizing about the importance of Central Asia, but another possibility, climate change, needs further study before any firm conclusions can be reached.

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Frank's study traces the first 3,000 years of a single world system that is ancestral to the modern world. Correction of the errors or misconstruals of archaeological or historical detail is not very interesting—these will sort themselves out with time. The more fundamental issue is whether Frank is describing a historical reality.

Frank envisions a hierarchical structure of capital accumulation that provided the central connection between center and periphery, shifting surplus from the latter to the former by largely market forces and only secondarily by "political" ones. Although maintaining a continuous identity over at least the past 5,000 years, the world system has experienced modest expansions/contractions in extent and cycles of greater and lesser prosperity. The ambiguities here are enormous, not least regarding Frank's assertion that the ancient world was qualitatively similar to the "modern world-system" and may be analyzed in similar language. Instead of reengaging this perennial debate, the following comments address questions of method.

Not all regions in contact with each other should be considered as belonging to a single world system. For example, the presence of silk in burials at Sapalli-depe (southern Uzbekistan [Askarov 1973]) implies a probable connection with China at the beginning of the 2d millennium B.C., the same period in which cloves from Southeast Asia appeared at Terqa (Syria [Buccellati and Kelley-Buccellati 1983]). Central and West Asia in turn enjoyed links with the Eurasian steppes (e.g., Chernykh 1992), South Asia (e.g., Hiebert and Lamberg-Karlovsky 1992), the eastern Mediterranean (e.g., Millard 1977), and, indirectly, the rest of Europe. But the archaeological evidence hardly warrants reference to a single world system that encompassed all of Eurasia. If the world-system concept is to have any point, the analysis must reveal mechanisms of interregional relations (center-periphery, uneven accumulation, unequal economic/political power, dependency) that are distinguishable from the more traditional ones such as trade, diffusion, and migration.

Recognizing this, Frank attempts to demonstrate the reality of a single world system by pointing to interregionally synchronous developmental cycles. One difficulty with this is the precision of synchronisms. Frank at several points indulges in special pleading to fit the

chronological pieces together into a single A/B episode and even finds virtue in local A phases within a world-system B phase. Another difficulty is logical circularity: the test for identifying a world system is the same as the analysis of its contents. Frank admits that factors such as global climate change may produce similar synchronisms of regional developments. But until he exposes the mechanisms that endorse the world-system identification, he is assuming the analysis before he undertakes it.

Even the identification of A/B phases remains contentious. Frank uses as indices demographic, economic, and political trends. These indices are a mixed bag. Mesopotamia illustrates the problems. Settlement surveys indicate peak urbanism early in the 3d millennium and deurbanization over the next two millennia (Adams 1981). During this time, the economic and political landscape fluctuated between integration (Akkadian, Ur III, Old Babylonian, Kassite periods) and disintegration into competing polities (Early Dynastic, post-Akkadian, Isin-Larsa periods). Levels of political intensification and control of economic production and circulation did not correspond to political fluctuations in any clear-cut way. The bureaucratic machinery of the Ur III state directly oversaw many aspects of the regional economy; the Isin-Larsa and Old Babylonian states encouraged private enterprise and leased crown agricultural land and herds to entrepreneurial firms; the Early Dynastic III economy involved a variable mix of palace, temple, and lineage production. The permutations of political economy all permitted the continued flow of foreign commodities. Indeed, the competing Early Dynastic III city-states seem to have enjoyed a larger volume of trade than the subsequent Akkadian empire; similarly, the volume of trade was larger during the Isin-Larsa period than during the previous Ur III period and declined again with the Old Babylonian period. The inverse correlations of trade and regional political integration perhaps arose from tribute satisfying demands for exotic materials during periods of integration and from conflict stimulating demand for exotic wealth in legitimizing displays of elite status.

The incongruences between settlement pattern, politics, and economy imply that any single measure is inadequate to define an A/B cycle. Frank's reference to the "tests" based on Chandler's city census and to the changing rates of warfare is therefore vacuous. Similarly, regional political integration or empire does not guarantee greater capital accumulation, and volume of trade and the wealth that trade might generate do not necessarily correspond to degree of urbanism, political structure, or militarism. Indeed, any formal definition of the A/B dichotomy is bound to fail because of the extreme heterogeneity of the defining terms.

In the end, Frank's model overreaches by insisting on the modernity of the ancient world, by failing to elucidate the structural connections between regions, and by settling for overly eclectic criteria for relevant evidence. Even so, Frank does point to potentially significant avenues of analysis; if his basic goal is to challenge archae-

ologists and ancient historians to rethink their perceptions of the past, then perhaps he is successful.

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Having seen and discussed various versions of the 5,000-year-system model proposed by Frank, I am pleased to find it in a journal where it can be discussed by archaeologists and ancient historians. While I agree with the most general argument of the article—that there is a continuous global system linking much of the Old World from the 3d and perhaps the 4th millennium B.C.—and while it is surely interesting to investigate the possibility of the existence of A and B phases, it ought to have been made explicit to what such categories refer. Expansion and contraction are not sufficient summaries, and a more processual model, not unlike those found elsewhere in Frank's work, should have been provided. It is because the model is not made specific enough that the indicators can be so vague. City size, population size, and evidence of economic growth (difficult indeed to discern in this discussion) as they appear in a few secondary works are not really adequate to such a discussion. Even if, for example, the city-size category were empirically reliable (and Frank is clear regarding the unreliability of the data), we ought to have an argument about the relation between economic expansion and the formation of central cities and urban ranking. Such an argument might account for phenomena such as Mexico City, certainly not the center of the world system, and the Indias and Chinas of the modern world. I have argued on several occasions that expansion in the center is most often and systemically linked to political decentralization rather than the reverse and that centralized empires were often a symptom of slowdown or even decline. These processes appear to contradict the associations established here for A and B phases.

The foundation of Frank's argument for the antiquity of a unified world system in the Old World is the apparent evidence for a world market and a world network of exchanges. We have argued in similar terms (Ekholm 1979, Ekholm and Friedman 1979), but I would like to suggest a certain refinement here. It is not so much a commodity market as the existence of a form of capital accumulation—the accumulation of abstract wealth—that is the foundation that we stipulated. This accumulation might occur in a multiplicity of forms, from different constellations of markets to more centralized state patterns. Frank is rather excessive in classifying Weber with the primitivists and substantivists. While Weber, to be sure, argued that capitalism never emerged as dominant in the ancient world, his question was related to the confrontation of different kinds of strategies, including the accumulation of abstract wealth. This is very different from the Polanyi and Finley argument that capitalist economic goals and therefore processes simply did not exist in the ancient world.

One of the main problems arises with respect to the

unclear treatment of A and B phases. Frank appears to assume that such phases ought to be more or less homogeneous, that is, that every actor in the system should follow the same rhythms. He emphasizes this in his noting that Southeast and East Asia today are somehow out of phase with the dominant B-phase tendency. He also suggests that the B-phase contraction is the source of shifting hegemony in the system. We have argued, on the contrary, that it is decentralization and the tendency toward shifting hegemony that is the cause of decline and crises of accumulation in general. Even if the entire system is in a contraction phase, this does not determine the distribution of accumulative advantages; some areas may always be expanding at the expense of others. Maurice Lombard's important studies of the Mediterranean and the Middle East argued that Rome and Asia were systemically linked in inverse cyclical relations which accounted for the long-term repetitive shifts in hegemony between the two areas (Lombard 1975).

The assumption that decentralization and/or the fall of empire is a symptom of a B phase does not hold up under scrutiny. The very formation of the "first" city-states of Early Dynastic Mesopotamia is the product of a demographic implosion related to increasing competition over (decentralization of) control over land and trade. Rathje argued quite a few years ago that the decline of the Classic Maya was not an economic decline but a transition to a more decentralized commercial expansion. Similarly, the transition from Hallstatt to La Tène "cultures" in Europe is not a fragmentary decline but might be a decentralized expansion of trade and production. The Eastern Chou and Warring States period in China is usually associated not with B-phase decline but with economic growth.

In sum, I think that the description of the world system in terms of parallel cycles of expansion and contraction is far too general, homogeneous, and vague to enable us to understand the nature of the connections assumed to exist and to define the world system. More could certainly be coaxed out of the data if the model were made more specific.

Frank makes an interesting suggestion with respect to the relation between large-scale political-economic change and cultural change. Jasper's "axial age" is here seen as a systemwide response to cyclical decline. This age, from 800–200 B.C. for Jaspers, is reduced to 800–550 by Frank to fit his B phase. It is characterized by the emergence of universal religions and philosophies and the crucial concept of transcendence. In fact it is also characterized by the emergence of science, theater, and a strong tendency to secularization and individualization in the Mediterranean and large parts of the Middle East, a process that might be likened to the emergence of a kind of modernity (Friedman 1991, 1992). Here, while the globality, however uneven, of this change is well taken, its internal properties cannot be accounted for by the economic cycle itself (this would require us to explain why it did not occur previously). By specifying the nature and degree of commercialization and the transformation of political structures and conditions of

social existence, we might be able to account for what is more like a global trend or structural transformation (reversible, of course) than a global cycle.

These comments are meant to urge clarification, but they are made on the assumption of general enthusiasm for an approach that I hope will make increasing sense out of our species's history on this planet before we are replaced in a yet longer global cycle.

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Frank puts forward two concrete claims: that, as early as the late 4th or early 3d millennium B.C., the Near East and adjacent areas of Europe and Asia formed an interconnected economic unit, one which expanded to encompass most of Eurasia over the following 3,000 years, and that this Bronze Age world system underwent cyclical, synchronic patterns of prosperity and decline (A and B phases, respectively). These claims might be accepted on one or both of two grounds: because they are supported convincingly by the empirical evidence afforded by the historical and archaeological records and/or because they make sense (that is to say, they are supported by a coherent and convincing theory of the processes governing archaic societies). Frank's essay fails to establish either of these grounds persuasively.

Archaeologists have various methods, none of them beyond controversy, for determining the extent of trade between societies, assessing changes in their prosperity, and establishing dating frameworks for those events. Frank, rather disarmingly, absolves himself of the need to examine such evidentiary matters directly: he is a sociologist, content to accept the conclusions of experts directly engaged in the study of the record. Unfortunately, when the experts disagree about details (as they must, given what they have to work with), he feels free to pick the version he likes best (instead of realizing that he must get to the bottom of the issue); when the experts hedge their conclusions, he feels free to disregard their caveats; when they are unsympathetic to the premises of the world-system approach (e.g., Harding 1984), he ignores them altogether. Such selective culling of secondary sources merely propagates error through the intertwined hypotheses.

Speculative theory building might still be useful if it could lead to fruitful reexamination of the archaeological record. Frank notes that archaeology provides difficult evidence, but conceding difficulties does not remove them. He is so insensitive to the archaeological record's constraints that his hypotheses cannot possibly be tied to evidence. His 200-year A and B cycles, for example, would be susceptible to evaluation only on the condition that the general chronology of events could be specified more tightly. If one could plot changes in city sizes or regional demographic densities or trade volumes at, say, 50-year intervals in a series of adjacent regions, Frank's proposed cycles would be imaginative and useful heuristic constructs (even if they were dis-

proved). Unfortunately, more than a century of archaeological research primarily devoted to refining chronologies has given us time intervals that, at best, match the length of those cycles (see, e.g., the various regional surveys in Ehrich 1992), and there is no realistic prospect of a breakthrough to greater precision. Constructs that are artifacts of the evidence cannot be useful for the critical examination of that same evidence.

No one doubts that some areas of Frank's Greater Near East co-prosperity sphere were in fact engaged in commerce such that their fortunes were somehow, somewhat, interconnected. What is in question is the geographic extent of that web and the degree to which the fortunes of its component parts varied cyclically. This essay's special pleading does nothing to advance discussion of the empirical aspects of these issues.

Frank's discussion of the theoretical side of these issues also leaves much to be desired. He does not discuss but merely dismisses a "primitivist-substantivist" view of ancient political economies. But what of the primitivist-formalist alternative (e.g., D'Altroy and Earle 1985, Brumfiel and Earle 1987) to Frank's modernist-formalism? This view would hold that markets existed but were limited in scope and that elite groups maximized their economic self-interest but tribute was more important than trade as a source of their capital accumulation. An approach along these lines would go a long way toward explaining the many anomalies Frank is constrained to treat as exceptions. If tribute is the key to the political economy, then one polity's difficulty constitutes another's opportunity. Frank is aware of this possibility (as in his mention of "musical chairs") but does not seem to recognize that such countercyclical phenomena contradict his generalized swings of prosperity and decline. Widespread declines and revivals certainly occurred at times (the eastern Mediterranean at the end of the 2d millennium is a good example that Frank makes much of), but they are tied to historically specific circumstances and should not be elevated into a general pattern.

Indeed, the greatest defect of Frank's essay is that it gives the reader no reason to suppose that such a general pattern should exist. In his conclusions he turns briefly to the demographic or climatic causes that might underlie his A and B phases. He provides no evidence for them (apart from the usual selective quotation of secondary sources), and he seems to be aware that such factors would not constitute adequate explanations, that a common "warming can affect agricultural possibilities in arid and humid . . . areas in exactly opposite ways," but he has no alternative to suggest. Empirical generalization, to the extent that it ignores variability, is a poor foundation for theory.

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The initial ideas concerning world systems, developed by Wallerstein, Amin, and Frank himself, were designed

to elucidate a specific problem: the creation of the modern world whereby Europe was enriched and much of the southern hemisphere systematically impoverished. The problem was well defined and had a direct bearing on contemporary issues; furthermore, the processes of colonialism and capitalism, such as those of capital accumulation, could be understood through existing theoretical frameworks (see the introduction to Wallerstein 1974). In the case of the "modern world-system," the problem prompted the model. Now there is a danger of the model's helping to shape history in its own image. Frank's article is based on the assumption that if a 5,000-year-old world system can be seen to exist this in itself will be of interest. I need more convincing that the world-systems framework is a useful one.

I do not, however, want to throw out the baby with the bath water. On an empirical level Frank (along with others) has identified an important problem: many areas of Eurasia have been linked through the movement of materials and people over long spans of time, and these links may have had an important impact on local developments. Although the existence of interconnections is clear, their nature is not. Frank argues for some form of capital accumulation from the Bronze Age onwards, with a system of "interpenetrating accumulation" linking various cores and hinterlands. He goes farther to suggest a direct historical connection between the Bronze and Iron Age world systems, on the one hand, and the modern world system, on the other.

Here it seems that he is tackling an old problem in a new guise: the rise of civilisation itself. There is an implicit argument that the Bronze Age, with the rise of cities, elites, and regular long-distance trade, set up a structure that was then unique in world history and has stayed with us ever since. However, archaeological evidence demonstrates widespread connections and movements of material from the late Palaeolithic onwards. An explicit argument is needed as to why the Bronze Age world system formed the basis for the modern world. As far as this argument is advanced in Frank's article, it seems to revolve around the notion of the accumulation of value and/or surplus. It is implied that forms of accumulation were institutionalised in the Bronze Age in a new way. The aim thus seems to be to provide a quantitative and economic definition for the term "civilisation." However, there is also a series of confusions, the most crucial of which stems from the use of the terms "surplus" and "value."

To simplify crudely, it is possible to say that since Marx people have used "value" as a means of looking at the manner in which changes in quantity, deriving from production, exchange, and distribution, turn into changes in quality, in terms of social structures, styles of life, and the uses to which material things are put in the social process. The structure of the accumulation of value, its spatial distribution, and the periodicities of its use can all be quantified and linked to their social and political consequences (see Harvey 1989). However, Frank presents no argument as to the transformation of quantity to quality, and we are left with the question whether quantitative changes in the economy over the

past 5,000 years all had the same social effects and what the nature of these effects might be. Not only are the theoretical bases of the approach unclear but it is uncertain whether the methods of measuring quantitative changes can ever be usefully applied. Early in the article Frank mentions Kondratiev and the cycles associated with his name, which immediately brings to mind graphs of the movement of prices (see, e.g., Braudel 1984: fig. 9). However, the main quantitative data advanced, such as they are, concern the number and size of cities, which are far from being a straightforward index of the health of the economic system. The nature of accumulation is seen to be central to changes within the system, but no measures of accumulation are offered. In short, I feel that both the theory employed and the methods it necessitates are based too much on the present, where the nature of the economic process and its quantitative changes are comparatively well known and some of the links between quantitative and qualitative change can be specified.

Should we say that history at this scale from the Bronze Age onwards should not be written? There is a real problem to be tackled here; from an early period, which may or may not start in the Bronze Age, a series of interconnections in Eurasia was at least as important as local developments. The use of world-systems theory may lead us onto the wrong track, producing arguments about how similar Bronze Age societies were to the present and tempting us to use methodologies which work well for the past few hundred years but cannot work for periods five millennia ago, with their myriad uncertainties of dating and quantitative analysis.

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Frank rightly suggests that a conventional archaeologist would have considerable qualms about charting "Bronze Age world-system cycles." He lists many of the difficulties involved—uncertainties of dating, ambiguity of both archival and site evidence, uneven coverage of the different parts of the Bronze Age "world"—and to some people they will seem insuperable. We are therefore considerably in his debt for giving us this article and simultaneously providing us much food for thought. There is no doubt that it will serve as a landmark and that his "tentative findings and propositions" will be extensively examined and, perhaps, revised.

A number of difficulties face the European prehistorian in attempting to assess Frank's ideas, not least the fact that he has very little to say about Europe itself. Unfortunately, several of the key items that he quotes are unpublished. For me most difficult, however, is the fact that it is hard to know whether the statements he makes are true or false; there is little in the archaeological record on which to base a judgement, and since he relies mainly on the archaeological statements of world-

systems-theory enthusiasts, paying little attention to contrary opinions, we are not really much farther ahead.

In some ways this application of grand theory is rather like the concept of the Grand Unified Theory for physicists: if it can be correctly formulated, it will provide a framework for understanding all the remaining phenomena of the academic universe that we inhabit. It is likely, in my view, that the truth is considerably less tidy than this, and in the context of an archaeological analysis of the Bronze Age one would need to be convinced that the cyclical movements really took place on a global scale. Frank's criterion for identifying the existence of a world system is that "no part of the system would be as it is or was if other parts were not as they are or were," and he lists trade connections, political relations, and the sharing of cycles as "criteria of participation in the same world system." In the part of the Bronze Age that I know best, the "barbarian" world of Europe and its relations with the Mediterranean, it is common knowledge that all of these are controversial matters, and the controversy is certainly not dispelled simply by quoting from Kristiansen's writings, openly partisan in approach.

How can one answer such questions? Are the data adequate? This will of course depend on time and place, and different observers will arrive at different answers. It is certainly possible to believe, as Frank evidently does, that the evidence from the Near East, Mesopotamia, and India is strong enough to support the idea of economic and political cycles in the Bronze Age. I am less happy about using Chernykh's work on metallurgy to legitimise a vast "Eurasian world system"; the issues involved in the spread of metallurgy are complex and the mechanisms involved in the transfer of knowledge not necessarily best described in "global" terms. In the case of Europe, we are mainly dependent on archaeological site evidence—desertions, destructions, evidence of trade from artefacts, and so on—and it is correspondingly harder to create interpretations that are both supported by adequate data and sufficient to the explanatory purpose for which they were created.

Were there cycles at all? The idea of a cyclical relationship between Europe and the Mediterranean at the end of the Aegean Bronze Age was first formulated by Bouzek (1969: 86 fig. 31) in the demonstration that as the number of settlements in Attica declined around 1200 B.C. the number in central Europe rose dramatically, and vice versa in the 8th century B.C. Bouzek's interpretation of this phenomenon is a straight historical one, relying on a combination of climatic and demographic factors, but it is nonetheless a kind of ancestor to the more formalised theories of cycles which this article presents.

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Frank's reconstruction presents a number of methodological and theoretical problems. At issue is neither

disqualification by “the profession” nor the timidity of specialists but whether data are used accurately and in context. Dependence on secondary sources leads Frank to repeat errors and misconstrue evidence. For example, indexing the rise, collapse, and incorporation of various societies into a world system according to city censuses or quasi-historical accounts of warfare is simplistic. Taking such extrapolations uncritically overlooks the complexity inherent in archaeological and historical analyses. Nor is it clear why changes in urbanism or conflict at the local or regional levels necessarily signal the development of intersocietal structures.

The major theoretical problem with Frank’s argument is that it equates correlation with causation. Frank is at pains to correlate local sequences of rise and collapse. Some of these correlations likely represent panregional phenomena; others, however, are epiphenomena or simply illusory. Establishing and dating local cycles are complex archaeological problems, demonstrating interpenetrating causal relationships even more so. For example, the panregional phenomena forming A and B cycles are subject to conflicting interpretations. The Uruk settlements in Iran, Syria, and Anatolia are regarded as colonies by Algaze (1989) but as refugees fleeing southern Mesopotamia by Johnson (1988–89). *Contra* Ratnagar and Shaikh, Shaffer (1982) doubts whether Harappan civilization was substantively affected by contacts with Mesopotamia and the Gulf and whether Harappa should even be regarded as a “state.” Discussions of collapse also show how important elements of societies persisted through B phases; curiously, Frank’s approach includes neither these discussions nor the fundamental contributions of Braudel (Braudel 1980; Lamberg-Karlovsky 1985, 1986; Yoffee and Cowgill 1988; Tainter 1989; Willey 1991).

That there were different kinds of “trade,” unequal development, and progressive accumulation of power in cores during antiquity is clear, but except for metals Frank does not explore what materials were being moved, when, by whom, and for what purpose. In contrast the discussions of Edens (1992) on trade and consumption of utilitarian shell and grain in the Gulf and Mesopotamia, Kohl (1978) on chlorite vessel production, exchange, and consumption linking Mesopotamia to eastern Iran and Afghanistan, and Joffe (n.d.) on Mediterranean crop production in the Levant and consumption in Egypt show how trade in particular goods resulted in their valuation and commodification and the impact on societies and elites of their production, exchange, and consumption. Without reference to mechanisms and pathways of material transfers, Frank is left only with correlations, while explanation is homogenized to “total accumulation.” “Total accumulation” is an essentialist feature acting as a prime mover if not a *deus ex machina*. It is either a reified notion of society or a partially articulated theory of elite behavior that ignores the contingent roles of history and culture in social evolution. Sanderson (1991) has shown that world-systems theory is a form of evolutionary theory that exclusively emphasizes exogenous factors. Frank’s use of it ignores the in-

ternal features of local sequences and acts as a latter-day form of diffusionism. His global perspective breaks down boundaries between units, but at its core there is only a basic Marxian notion of a ruling class. By taking local and regional phenomena of rise and collapse out of context he deliberately sacrifices explanation in contextual terms in favor of a reductionist and homogenizing framework.

Contingent factors of local history and culture affected social evolution and the development of intersocietal formations such as world empires and world systems (Kohl 1984). The xenophobia of Egypt and the cultural competition between Assyria and Babylonia conditioned local, regional, and, ultimately, world development. The pivotal roles of religion and ideology in the Inca and Aztec expansions have been discussed by Conrad and Demarest (1984). Helms (1988) has shown that "trade" is frequently initiated by and for religious-political elites whose goal is not "total accumulation" but the accumulation of specific kinds of power. A concept of power comprised of social, religious, economic, and political aspects provides a more satisfying basis for explaining ancient trade and the evolution of intersocietal interaction than a purely materialist framework (Runciman 1982, Mann 1986). Cycles existed in the ancient world as they do in the modern, but little suggests that the world system was tightly integrated prior to the 1st millennium B.C. and the advent of panregional empires.

World-systems concepts have been diluted and reformulated by historians and archaeologists but retain an undeniable appeal. It could be argued that their usefulness is less interpretive than heuristic. Frank's contribution is thus a challenge to specialists to look outward. It is healthy and necessary for nonspecialists, particularly those with strongly developed theoretical perspectives, to contribute to debates that frequently degenerate into squabbling over minutiae. But Frank cannot leave the details and explanations of local changes to the specialists or ignore existing discussions. His earlier work has contributed considerable insight into the present. Without greater attention to the detail and character of the past, the structure and process of the ancient world and its connections with our own will not be revealed.

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Frank's paper has great *potential*, and his knowledge of world-systems theory has few counterparts. But the attempt to interweave elements of that theory into the vast area of "Afro-Euroasia" and the Mediterranean throughout the Bronze Age is very weak at the seams, seriously overextended in time and space, often self-indulgent, and replete with "factual" errors. The last complaint is minor and obviously beyond any single writer's control, but fuller collaboration with an archaeologist rather than reliance on comments from those

cited in the acknowledgements (all of whom, by the way, work in western or southern Asia, not in the Mediterranean or North Africa) would have benefited this article immensely and made it required reading for any Bronze Age archaeologist. As it stands, it makes a critical, innovative, but often misleading contribution when it seeks to treat areas beyond ancient western Asia and the Indus, and I think it is wrong to try to include the Lapita phenomenon and Oceanic prehistory in this particular argument (see e.g., Allen 1984, Gosden et al. 1989, Bellwood 1987, Bellwood and Kuhn 1984).

Although Frank's paper purports to treat everything from the Mediterranean to South Asia, the focus throughout is on Mesopotamia, central Asia, and the Indus, only occasionally on Europe (Kristiansen's work). Even then, the discussion of the "ancient economy" debate overlooks Yoffee's (1981) key article and most relevant studies on the Mediterranean (e.g., Earle 1985, Knapp 1985). The Mediterranean, which plays a key role in *any* consideration of a Bronze Age world system, receives very limited coverage (brief references to the admittedly superb paper of Sherratt and Sherratt [1991a; see now 1993]). Frank's focus on western and central Asia to the exclusion of the Mediterranean (South Asia does not really fare much better; Wheatley 1975, for example, is not considered) results from his reliance on Edens and Kohl rather than on any past social or politico-economic reality. Without fuller consideration of the *Mediterranean* world, "the Bronze Age world system" is far less comprehensive than it should be. *Contra* Edens and Kohl, for example, the Bronze Age world system extends not just to the eastern but to the *western* Mediterranean (see Bietti Sestieri 1988, Muhly, Maddin, and Stech 1988, Knapp 1990; on the eastern Mediterranean see Cherry 1986, Muhly 1986, Manning 1993, Knapp 1993).

Frank acknowledges the difficulties with the Iron Age, the treatment of which is very generalized, underreferenced, and misleading compared with that of the Bronze Age. It would have been useful, certainly, to have a statement of the problems with the Iron Age (following the "B phase, 1200–1000 B.C."), but otherwise its inclusion in this study seems entirely inappropriate. In fact, the section on the "B phase 1200–1000 B.C." contains an unfortunate mixture of "fact" and fiction (e.g., the Dorian "invasion" is mythological, and one must be skeptical of any invasion or migration postulated as a primary cause of cultural change, as Frank himself points out). Furthermore, one cannot just cite James et al. (1991) without pointing out how controversial this study is, even if it does highlight problems associated with the close of the Bronze Age (see reviews and reply in *Cambridge Archaeological Journal* 1[2] and 2[1]).

Other, more minor points of omission or commission include the following: (1) The issue of tin sources is much more complex than Frank portrays it (for an overview of material from Asia to England, see Muhly 1985; on Taurus tin, see articles by M. Hall and S. Steadman, A. Yener and M. Goodway, E. Pernicka et al., and L. Willies in the *Journal of Mediterranean Archaeology* 4[2] and 5[1]). (2) Wood and other organic materials were

also traded widely throughout the eastern Mediterranean (see detailed references and discussion in Knapp 1991). (3) The trade in silver, units of account, etc., was also common in the Aegean, especially in the Cyclades (e.g., Stos-Gale and Macdonald 1991). (4) Any discussion of the development and impact of metallurgy in western Asia during the Bronze Age *must* incorporate discussion of Cyprus (see Muhly 1986, 1989, 1991; Knapp 1986, 1988, 1993).

Frank has ingeniously examined Bronze Age cycles of production, consumption, and exchange in a manner that I fully commend and with a scope that will be enviable to most archaeologists who work in these regions. He is fully aware of the shortcomings I have belaboured here and rightly states at the outset that simply outlining the extent of the Bronze Age world system is enough. My criticisms stem from the fact that, with collaboration, this article could have been a major contribution to Bronze Age archaeology throughout the Old World and from the belief that the role of the Mediterranean in the Bronze Age world system has been seriously underemphasized. The latter is the fault not of Frank but of the archaeologists he has consulted. For example, whoever told Frank that Childe has been "discredited by the profession" is seriously misguided him/herself (cf. Sherratt 1989). It is now time for someone to take up the challenge of filling the gap left by Frank, a role that the Sherratts have already taken very seriously (1991a, 1993; Sherratt 1992).

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Frank has written a bold, explicitly provocative article that is meant to stimulate prehistorians to seek interconnections among disparate cultures and to detect cyclical contractions and expansions of economic activity over vast areas of Africa and Eurasia from the beginnings of the Bronze Age through the end of Classical Antiquity. Insofar as his essay performs this function of forcing archaeologists to climb out of their trenches and search for broader patterns of interaction stretching far beyond their local areas of research, it is to be applauded; insofar as it fosters the illusion of a real, empirically ascertainable prehistoric Bronze Age world system orchestrating the course of world history, it must be received more critically. At one level, the problem is one of reification: Frank's world system has become real and not just a model that is useful when it clarifies understanding and expendable when it does not. His "reality" wags the tail of the evidence when, for example, he writes that an earlier identification of a B phase from 1700 to 1500/1400 B.C. "implies a previous A phase." At another level, Frank's model relies on the highly selective utilization of largely secondary if not tertiary sources that attempt to reconstruct broad patterns of interaction from very incomplete and problematic materials, primarily the archaeological record. The metaphor

becomes that of sand castles constructed on top of one another or a house of cards. How many levels can be built before everything collapses in on itself?

Frank, of course, is aware of these difficulties and is honest enough to spell most of them out at the beginning. Nevertheless, he remains undeterred from the formidable task he has set for himself: the delineation of cyclical A and B phases of expansion and contraction across an essentially ever-expanding *single* world system (in the realm of believers, Frank is a staunch monotheist) that stretch back into the mists of the 3d if not the 4th millennium B.C. There are limits to an admirable boldness of scholarship, and Frank's recognition of the severe methodological and epistemological problems that he is forced to confront does not excuse him or make more credible the story that he tries to relate. I am sure that other commentators will criticize Frank on one or more of these recognized difficulties, particularly those relating to chronological uncertainties, and I do not want to appear as one more tiresome gainsayer. Again, it is salutary for a nonspecialist to try to reveal broad patterns of economic decline and resurgence to more timid archaeologists mired in the complexities of their data. One scholar's parochialism, however, may be another's justifiable caution. Frank recognizes the difficulties but somehow fails to accept their gravity.

Let me just briefly discuss two underevaluated problems: First, what is not known archaeologically is frequently much more impressive than what has been discovered. Many areas or regions remain essentially archaeological *terra incognita*. The decline of settlement in southern Turkmenistan at the end of the 3d or the beginning of the 2d millennium B.C. was interpreted as part of a broad pattern of settlement abandonment throughout eastern Iran ("prehistoric Turan") until Soviet researchers in the early '70s began to document massive Late Bronze settlements farther east in Bactria and Margiana. A similar situation characterizes our understanding of settlement patterns today in most of northeastern Iran (and here one should observe that our lack of understanding is not soon to be rectified for obvious political reasons), particularly on the Gurgan plain but extending northeastward onto the Meshed-Misrian plain of southwestern Turkmenistan. Hundreds of prehistoric sites, some exceptionally large, have been recorded for these areas, but practically none have been more than superficially investigated or excavated. These gaps in the record must be appreciated before one begins to paint one's broad canvas. Secondly, archaeologists often use terms such as "trade" in imprecise or contradictory ways. By no means does an archaeologically documented movement of materials constitute evidence of market exchange or the economic activities most conducive to Frank's cyclically expanding and contracting world system; such materials can move by a variety of means which have different social and economic consequences. Whenever possible, one must try to get a handle on the scale or quantity of materials being exchanged. Archaeologists notoriously inflate the significance of their own discoveries, and the literature

is replete with impressive-sounding phrases such as “massive movements” or “large-scale exchanges” which, when examined critically or from perhaps an excessively empiricist account, almost disappear. Resort to an “invisible trade” in perishable commodities may be justified, but it may also be invented. At the very least, it reflects the limitations of dealing with archaeological evidence.

If one remains more of a skeptic than a believer, the question becomes: Is Frank’s model useful? Despite my admiration for the boldness of the endeavor, I am afraid that my answer is largely negative. With many significant reservations, which I need not reiterate here, I have argued for the utility of a world-systems perspective for the Bronze Age. It all depends on the looseness of the model one employs and its scale. Frank’s terminology and search for evanescent cycles imply a far greater articulation of economic activity over vast areas than I believe characterized the Bronze Age, at least in its beginning phases. Much depends upon our understanding of the production and exchange of metals, and Chernykh’s work is critical, with real differences emerging possibly in the 2d millennium B.C. But here too I am much more impressed with what needs to be done than with what we now know (e.g., our understanding of the exchange and production of tin-bronzes). Frank’s effort, unfortunately, misleads us in seeing all rises and falls as *systemically* interconnected. History should be written on a grand scale; cultures are open, not closed, systems, always interacting with other cultures and participating in shared historical processes larger than themselves. However, there is no god in the form of a world system that directs all these processes.

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Frank has set out to “test” the existence of a single world system originating in the Bronze Age by demonstrating empirically a common pulse of economic ups and downs running through the system. This apparently simple and yet enormous task is in itself an important contribution to the widening discussion about world-system theory. Most case studies up till now have encompassed only part of the system, assuming wider regularities. Thus the primary “test” of the existence of a single ancient world system going back 5,000 years is indeed the demonstration of cyclical regularities in time and space, defining its geographical and chronological limits. I shall therefore comment upon the interpretation of A and B phases from the perspective of the European Bronze Age, as it represents the core of the argument. (I leave it to others to discuss the geographical limits of the system.)

While I can recognize the first A phase of the 2d millennium, which corresponds to the first flourishing of a real Bronze Age, employing tin, there is no reason to

introduce a B phase from 1750 to 1600/1500 B.C., which according to the most recent datings represents the Mycenaean shaft grave period and the flourishing of central European Bronze Age cultures. The next A phase, from 1600/1500 to 1200 B.C., is recognizable. The subsequent "Dark Age" after 1200 B.C. is of course the nearly universal collapse in the Mediterranean and the Near East, but again it represents a flourishing period in central Europe in terms of agrarian expansion and metal production. What we see here, then, is a more complex picture in which centres and their peripheries need not follow the same pulse. Collapse in the centre may be followed by flourishing in the former periphery. The same applies to some of the A and B phases of the 1st millennium B.C. Thus the B phase of 800–550 represented continued commercial expansion and colonization in the Mediterranean, while there was decline in large areas in west-central Europe. This phase should probably have its end date changed to 600 B.C. Also, the two phases 450–200 B.C. might be lumped together as representing one B phase of Celtic migrations in central Europe.

Although European Bronze Age and Iron Age researchers will recognize the shifts identified by Frank, they may not in all cases follow the interpretation of them as A or B in the European "hinterlands." Ups and downs depended upon structural position within the world-system hierarchy of core-periphery relations. This suggests that further research should concentrate upon understanding the nature of these structural relations. Frank's work has provided a valuable platform for such discussions.

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Because of the enormous geographic and chronological scale of the analysis, I suspect that few archaeologists will feel comfortable with Frank's notion of a world system in the Bronze Age. In fact, however, Kohl (1989) has proposed a "world system" involving Central Asia, the Iranian plateau, and Mesopotamia, and Edens (1992) has attempted to link the "world system" of Mesopotamia with that of the Persian Gulf and the Indus Valley. Tosi and I argued for what Caldwell (1964) called economic "interaction spheres" uniting Central Asia, the Indus, the Iranian plateau, the Persian Gulf, and Mesopotamia within a market network (Lamberg-Kalovsky and Tosi 1973, 1975). The question remains: What were the precise "political-economic fortunes" that brought distinctive cultures into contact?

Even when large areas and several centuries are lumped—which, given the relative paucity of evidence, is essential for making a case for a Bronze Age "world system"—Frank's thesis remains highly strained. His essay and the recent writings of Kohl and Edens give the impression that both finished commodities and raw resources were being traded in bulk over vast stretches of Eurasia and that this commerce resulted in economic

dependencies, political hegemonies, and exploitative core-periphery relations. Only Edens (1992) has made an effort to quantify his argument, counting three items made of shell: lamp cups, cosmetic containers, and cylinder seals from around 3000 B.C. to the early 2d millennium. Without attempting to control for differences in areas excavated in the various time periods or the different functional contexts from which the shells were recovered, he concludes that this small number of items spread over the course of a millennium represents "transformations of the Mesopotamian political economy attendant on Akkadian and later imperialism" (p. 125). I applaud such efforts, but this grand conclusion seems to me to emerge not from the archaeological data but from what the written texts explicitly say.

Late-3d millennium texts from Mesopotamia speak of trade with Dilmun (the Persian Gulf), Magan (Oman?), and distant Melluhha (the Indus?). Yet archaeological excavations have not recovered a single Mesopotamian or Omani artifact in the territory of the other, nor has a single artifact of incontestable Mesopotamian origin been recovered from the Indus or from Central Asia. Thus the *archaeological evidence* for a "world system" is meager and tends to concentrate on a limited repertoire of elite artifact types—etched carnelian beads, Gulf seals, intercultural-style carved chlorite vessels, lapis lazuli, etc. At the same time, the *textual* evidence indicates that cereals, textiles, and copper were extensively traded, at least between Mesopotamia, the Persian Gulf, and Oman. Other materials, such as Magan onions to Mesopotamia, were traded but apparently in lesser quantity. Generously combining archaeological and textual data spread over a broad region and several centuries readily allows assumptions to become conclusions. What is needed is careful archival study in conjunction with a quantified study of an archaeological resource allegedly circulating within the "world system." Studying the texts from one period at one site (and preferably from one archive) is the approach that informed us that in the Ur III period the texts from Lagash, for example, deal primarily with the agricultural economy of the temple, those from Umma with the activities of merchants and state-run agriculture, those from Nippur largely with a private economy, and those from Ur with the manufacture of products (Gelb 1968). Prosopography can be of enormous utility; thus Larsen's (1982) study of Imdi-ilum, an Assyrian businessman residing in central Anatolia, is an example of capitalism in the 19th century B.C. that united this region with Mesopotamian communities to the south.

The ancient-vs.-modern-economy debate continues to turn on the outmoded notion that historical stages characterized by specific social/economic/political types succeeded each other in linear fashion. Gelb (1987:7) long ago argued that "it is impossible to speak of one type of economy to the exclusion of all others, be it temple or state or private." Wolf (1982) has advanced three modes of production—kin-ordered, tributary, and capitalism—without presenting them as evolutionary stages. That the three modes coexisted in the

Bronze Age is evident in the earliest systems of land tenure in Mesopotamia (Gelb, Steinkeller, and Whiting 1989). While such studies as Larsen's (1976, 1982) give clear evidence for the existence of capitalism, others, such as those of Saggs (1989) and Moran (1992), illustrate the coexistence of kin-ordered and tributary modes of production at a time when capitalism was being practiced.

While I see the ancient and the modern economy as sharing many attributes, I do not find the cyclicity that Frank addresses very convincing. Contouring world systems and embellishing them with a cyclical pattern is adventurous, and the scale of the analysis is dizzying. Perhaps today's economic world system, modern hegemonic rivalries, and the development of underdevelopment cause us to see the same features in the past. I have little doubt that they were there, but it was at so different a scale as to make them bear little resemblance to those of the present.

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Frank's cycles get shorter as time goes on. Is there a reason for this, or is it just that there is more information in more recent times? Is there any danger here of dignifying mere eddies in the current as distinct phases?

Is there a terrestrial bias here? I wonder if seaborne trade and connections are not being slighted. Was there not perhaps a southern crust (Indonesian archipelago, Indian Ocean, Gulf, Red Sea) to the world system, mirroring the northern rim that Chernykh illuminates? And did the East African coast not participate, as it did conspicuously after the rise of the Islam?

Does an expansion of metals trade and metal use indicate prosperity, or might it not represent military exigency, so much of the metal being destined for military use?

Doesn't relying on city population figures (necessarily of highly dubious accuracy) as phase indicators neglect or at least undervalue the preponderance of human experience? Cities may have waxed and waned for reasons other than economic upswings and downswings—security considerations, for instance. In the modern world cities sometimes swell precisely because the rural areas are impoverished and food from afar comes only to cities.

The debate with Wallerstein over whether anything before 1450 counts as a world system seems unpromising to me; call it ecumene or intercommunicating zone or world system or world-system, it existed and was until quite recently the cockpit of world history.

The 18th–17th-century B phase is no mere cyclical swing but the chariot revolution. Are technical advances, even those destined for destructive purposes, perhaps part of Frank's capital accumulation? At any rate, this particular B phase seems to me so strongly related to war that some mention of this is appropriate.

As he reaches classical times, Frank's resistance to Hellenocentric chronology seems to weaken. Ought one not look to China rather than to the Aegean and Persia? If I read Barfield correctly, when China was stable and prosperous the steppe peoples enjoyed their halcyon days, skimming surplus from it; when it could not produce enough surplus to satisfy them, they had to try to conquer it. Thus A and B phases ought to coincide fairly well between China and at least the eastern steppe peoples, and using Chinese history to define them does not lead one astray. One might say that by early Han times perhaps the world system had acquired a center and the center was China. If so, it would not be amiss to write as Gernet has written and to see the whole system as dancing to a Chinese beat.

If there was a Bronze Age world system with common cycles, I can see only three categories of plausible explanation for them:

1. A core that dominates the whole. I suppose one might claim China as a core by 200 B.C., but I don't see any cores for the whole system before that.

2. Climate change. Frank briefly considers climate change as a factor, and this strikes me as worth close investigation. Rainfall and snowfall are the keys: the quantity and regularity of precipitation have always imposed a powerful constraint on production in the ecologically marginal zones of northern China, central and southwestern Asia, and North Africa. Palynology can offer proxy evidence for precipitation history, and the resolution is often sharp enough for cycles that take centuries. If I remember correctly, Ellsworth Huntington tried to write Asian history with a climatic determinism at its heart, but he had no reliable data. Now there are data, and the plausibility of such a determinism can be tested. If I am wrong that there was no core, at least before 200 B.C., then it is possible that climate change in a core area rippled economically throughout. In other words, if the whole system was (at least at times) driven by ups and downs in a core, those ups and downs might still have had climate as an important driving force.

3. Disease/population cycles. Another possibility briefly recognized by Frank is that cycles of population growth and decline drove the economic cycles and that those population cycles were defined by the periodic eruption of systemwide epidemics. Crosby (1972, 1986), among others, has shown the historical consequences of new infections among populations lacking the requisite immunities. His examples all date from the past 1,000 years, but the mechanism may well have relevance to the Bronze Age. Sharp oscillations in population (and in labor force and extractable surplus) might therefore be defined by the rhythm of "virgin-soil epidemics" of measles, smallpox, typhoid, influenza, etc., acting singly or in groups. In the cases of the introduction of new infectious diseases into New Zealand, Hawaii, and the Americas, the decline lasted 120–150 years. Recovery came at more variable rates. How might similar phases (if presumed to exist) correlate with Frank's economic cycles?

Human history takes place in a crowded ecological context. Insofar as history shows cycles, their trajectory-

ies, periods, and causes may be determined by elements of that context as much as or more than by the affairs of human societies themselves. This may offend the *amour propre* of our species, but it is true.

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As an undergraduate at the University of Minnesota I was taught that scholars always worked from primary sources. Stringing together a series of quotations from secondary sources was acceptable for undergraduate research papers but entirely unacceptable in works of serious scholarship. What we get from Frank's paper is some idea of what scholars such as Chernykh, Edens, Kohl, Kristiansen, and Ratnagar have to say about his general topic. Where does that get us? Frank would like to believe that a world system (or world-system) was already in operation in the 3d millennium B.C., if not earlier. No evidence is presented to support this (or any other suggestion made in this paper); Frank simply thinks that it is an interesting idea and seems to feel that his readers should take it from there. Such a system might very well have connected economies over a very broad geographical area in the Early Bronze Age, from Troy to Harappa, but where is the evidence for (and against) such a hypothesis?

Frank admits that he knows little about the periods and areas he tries to cover. He seems to see his role as that of an agent provocateur, tossing out suggestions and providing a framework designed to promote future research. But such suggestions, if they are to be taken seriously, must (at least to some degree) be anchored in reality, in the surviving archaeological and historical record. Otherwise we end up with the proverbial scholar who reads three books on a subject in order to write the fourth.

Even on its own terms this paper cannot be taken seriously. In seeking support for a particular interpretation of a period of history it is always best to make reference to scholars with known expertise in that period. A number of scholars will find themselves here in quite unexpected contexts. Chernykh is certainly a scholar with recognized expertise in several different areas of research, but the end of the Late Bronze Age is not one of them. In support of the assertion that "the Mesopotamian region experienced a long peace from 1380 to 1331 B.C." (and what is that supposed to mean?), the reader is referred to a paper delivered at the 1992 meeting of the American Sociological Association.

In attempting to evaluate the implications of any broadly based hypothesis it is always best to proceed from the known to the unknown. If world-system theory is problematic for the 3d millennium B.C. as Kohl has argued, then what of the 2d and 1st millennia? It is not encouraging to be told that the period 1750–1500 B.C., was a major B phase "Dark Age." In light of the wealth

of material from Akrotiri (Thera) and Zakro and Knossos (Crete), the incredible treasures of the shaft graves at Mycenae, and the formation of the Hittite Old Kingdom in central Anatolia, with the Hittites pushing as far east as Babylon ca. 1600 B.C., it is hard to think of the period as a Dark Age.

Things get even worse in the 1st millennium, with a downswing put at ca. 800–550 B.C. This is the period of the great Phoenician expansion to the far west, the southern coast of Spain and the opposing coast of North Africa (but are we really back with Phoenicians in Britain?), of the reawakening of the Greek world in the age of colonization and the Orientalizing period (with the development of the Greek alphabet and other major intellectual developments), and the establishment of Assyrian control over eastern Anatolia, the Levant, and even the island of Cyprus.

I could go on, but there is little point in doing so. The world-system concept is a most intriguing one, and I look forward to the forthcoming book edited by Frank and Gills, but any theory, however intriguing, has to be evaluated in terms of the credibility of its supportive evidence. Frank refers to an individual shipment of 20 tons of copper, presumably during the Old Babylonian period. I find this inconceivable. The largest copper cargo from the Bronze Age known to me, that of the Ulu Burun ship that sank off the southern coast of Turkey about 1300 B.C., was approximately half that size.

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All interpretations of the processes behind the development of civilisations are to some extent visionary. Since many of our expectations were generated by writers of the stature of Max Weber and lie embedded in half-conscious assumptions of the uniqueness of the West and the development of capitalism, it is salutary when a serious attempt is made to write the story on new principles. It is especially encouraging to those of us with an interest in the Bronze Age when a distinguished modern economist is motivated to look at our material because of its relevance to the growth of the world economy as a whole. It is also timely; similar ideas are emerging to respectability among (a few!) archaeologists and ancient historians, and attempts are being made to overcome the fragmented picture created by disciplinary overspecialisation, the regional autonomism of the New Archaeology, and the surprising longevity of Polanyi's substantivism among students of ancient Greece (it is sobering to realise that R. M. Adams's [1974] programmatic paper on ancient trade in these pages was written nearly two decades ago).

It is valuable, therefore, to have the question framed in such comprehensive terms. This is not to say, however, that ancient world economies had anything like the coherence of the recent world system, and an initial search for systemwide cycles may be a heuristic exercise

rather than a realistic description. Indeed, it is the successive changes of coherence (by no means a secular trend) with the growth of scale and alterations in economic organisation that may distinguish different phases of development. The congruence of economic and political power in the Bronze Age, for instance, may contrast with a looser relationship in the earlier Iron Age and give rise to a greater measure of interdependence whose cycles are visible synchronically in both economic and political spheres; but even so the strikingly asymmetric development of the Mediterranean and Persian Plateau/Gulf sectors in the Bronze Age suggests shift (or alternation) rather than synchronism in its long-term pattern of growth, with an even greater degree of freedom for marginal (as opposed to peripheral [Schneider 1972:21; Sherratt n.d.]) zones of the system. François Simiand's terminology of "A" and "B" phases may be misleading if axiomatically assumed to describe the behaviour of all parts and phases of the Old World system rather than certain temporarily coherent phases of its development. Even more so, blocks of Kondratieff cycles may be discontinuous and characteristic only of limited periods. Cyclicity is not the sole criterion of systemic relationships and may divert attention from more fundamental structures.

The evidence which Frank cites of interregional linkages (often at a high-value/low-bulk level) is well taken, as is his discussion of capital accumulation within the core regions. So, too, are his attempts to integrate the history of (1st-millennium) steppe peoples and those of adjacent urban areas as more than arbitrary collision. However, in treating the 3d millennium as a similarly coherent macrosystem he underestimates both the independence of the margin and the large-scale spatial shifts in the core/periphery nucleus, with the Gulf and the eastern Mediterranean as to some extent complementary spheres of expansion. By the beginning of the 2d millennium, the eastern Mediterranean and central Asian interaction spheres had effectively bifurcated. While both had their ups and downs, it is misleading to look for close synchronicity between them—at least until the Achaemenid period, when eastern and western ends of the urban world again developed to a common rhythm. Even within a single sector, however, shifts of supply routes (as in the European "amber routes" around 500 B.C. and, on a larger scale, between west-central Europe and the Pontic steppes in the 3d century B.C., both in relation to Mediterranean urban consumers) created alternative and complementary extensions of the periphery, with local boom and bust. Fitting this local evidence to a pattern of systemwide cycles is inherently misguided, although it has its own logic which deserves to be spelled out.

An outsider's view highlights the relative immaturity of the archaeological literature as a summary of what is currently known and how it is interpreted. Large-scale information surveys are often literally old or antiquated in their presentation of material. (Many of Frank's major B-phase migrations might disappear on modern scrutiny.) Our periodicals are full of excavation reports and

hot-shot reinterpretations of the nature of archaeology but notoriously short on interpretive syntheses which can be reliably used by visitors from other disciplines. Popular atlases sketch the scope but do not give the detail. This paper is valuable not least in showing ourselves as others see us and in indicating what they need to have from us. Marx and Weber wrote from their knowledge of classics and ancient history, when archaeology had revealed only a fraction of what we know today. If contemporary economic theorists really do want to know about the Bronze Age, it is up to us to tell them.

Reply

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I thank all the commentators, especially those who agree to disagree or even to dismiss my suggestions altogether. I am tempted just to plead guilty as charged, for although hardly anybody agrees with me much, I am glad to agree with everybody. I am obliged, however, to rise to charges so wide-ranging and mutually contradictory that it seems unlikely that I could merit them all. If my article is, as one commentator has it, "far too general and vague," let us make virtue of the necessity of clarification to extend this inquiry another few steps beyond the limits I had set for this occasion. Because the CA☆-treatment format does not really permit the opposing parties to confront each other bi- or multilaterally, at least not in this round, I will try first to act as a sort of honest broker among them, summarizing as faithfully as possible their cases and underlining their quite contradictory positions on several issues grouped by category. (To complicate matters, some commentators also take seemingly contradictory positions on the same issue.) Then, for each category of issues, I will state, reiterate, or extend my own position, of course *parti pris*.

Most comments deal with the method or lack of it in my madness. However, the proof of the pudding is in the eating, and therefore this reply will proceed from the most specific and concrete taste test, issues of cycle dating and world system extent, to the most general and abstract recipes. I apologize for not quite following this pedagogical maxim in my article, but then most commentators do not do so either.

Cycle datings and phase identifications: Some challenged, most not. Barceló, Harding, Kristiansen, and Muhly offer welcome critiques of my cycle datings and/or phase identifications. Barceló contends that 1000–800 B.C. was not an A phase because Phoenician expansion occurred later. Harding and Muhly argue that for the same reason 800–550 B.C. would not have been a B phase, and Kristiansen expresses doubts, though his suggestion that it ends in 600 B.C. implies that he accepts it. Perhaps they have a point about the Phoenicians, but several defenses are possible: (1) Finding one or even

several participants out of step is insufficient evidence that the others were not marching together. (2) Somebody is almost always out of step, particularly in B phases, like some “newly industrializing countries” in East Asia today. Kristiansen appropriately suggests that collapse in the center may be followed by growth in the periphery, and I would amend this to “some part of the periphery.” (3) It is this being out of step that permits evolutionary transformation in and out of any system and center shifts in the world system. Therefore, the Sherratts’ suggestion that we are confronted by evidence of center shift is well taken, but such shifts need not be alternatives to rather than complements or indeed results of systemwide cycles. (4) I have already referred to my observation that every B phase since A.D. 1500 has generated some explorative venture (e.g., by the Pilgrims to America in 1620, by Captain Cook to Australia in 1770) that laid the basis for larger investment in the subsequent A phase (Frank 1978a). (5) Others have observed an increase in colonialism during B phases in recent centuries (Bergesen and Schoenberg 1980). (6) Might it not have been precisely the emergence of economic crisis after 800 B.C. that generated Phoenician exploration and new colonies in the Mediterranean?

Muhly and Kristiansen also demur at labeling 1750–1500 B.C. a B phase because of Mycenaean and Central European growth. I have expressed some doubts about the datings myself, but there is considerable supporting evidence from West Asia, which is what prompted Silver’s “Dark Age” label. McNeill, instead, asks for greater emphasis on war and destruction during this B phase, which also included the introduction of the war chariot, mostly from Central Asia. I agree and have treated this more extensively elsewhere (Frank 1992). Perhaps Mycenaean and other European growth in this period should be considered, along with the above-mentioned later Phoenician one, something of a B-phase crisis-generated development at the margin of the world system which then helped shift its center of gravity westward. I leave it to the experts.

Harding questions the B-phase label for 1200–1000 B.C. because of a settlement increase in Central Europe observed by Bouzek. The latter does, indeed, observe “complementary” opposites between southern and northern Europe in several periods, and I have referred to this observation, albeit obliquely, citing “contributors to Harding (1982).” Bouzek finds these opposites intimately connected with climatic rhythms. The Sherratts find phase contradictions over a wider area and suggest that looking for systemwide cycles is inherently misguided. However, the *existence* of a “dark age” after 1200 B.C. is among the least controverted of views, and the Europeanist Kristiansen comments that “after 1200 B.C. is of course the nearly universal collapse in the Mediterranean and the Near East.” There may be more legitimate controversy about the precise dating. In referring to the “controversial” dating of James et al. I only said that the case I had already made on *other* grounds “would gain in credibility to the extent that” their dating revision is well taken. Therefore, I do not see why

legitimate reservations about James et al.'s work (which I also noted) should or need detract from mine.

Discussion and revision, even rejection, of my identification and dating of other cycle phases would have offered a most welcome additional tasting of the pudding. Only Kristiansen offers any, and that from a European vantage point: He recognizes the 2000–1800/1750 and the 1600/1500–1200 B.C. A phases and suggests fusing the unusually short 450–200 B.C. phases into a single B phase marked by Celtic migrations in Europe. Is absence of disagreement on other cycle datings evidence of the good taste of my cyclical pudding, or does the archaeological truism “The absence of evidence is no evidence of absence” apply here as well?

The existence of cycles. For the archaeologist Lamberg-Karlovsky, cycling is adventurous and unconvincing. For the more climatologically oriented Harding, cycles exist but far from being systemwide are in opposite phases in different places at the same time, and it is questionable whether we know or even can know that (my) statements about them are true. McNeill questions whether increases in metals finds can be used as indicators of A phases, since they may be related to war (more associated with B phases). This is a serious reservation that I hope is largely unfounded, but I have no satisfactory answer and hope that Chernykh and other scholars can provide one.

McNeill attributes important causation to climatic changes but says that precipitation is more important than temperature and that population growth, in turn affected by disease, is more causative than derivative. I suppose that all of these are interrelated in turn, but for now I have to defer to McNeill's ecological expertise, which he combines masterfully with socioeconomic analysis. In his recent book (dealing primarily with a much later period) he observes in passing that “it may well be true that one of the reasons for the decline of ancient civilizations is environmental deterioration and consequent falling economic productivity” (McNeill 1992:74).

McNeill finds that my cycles get shorter over time and perhaps become mere eddies. I do not see this, despite my qualified suggestion of some shorter cycles between 550 B.C. and 100 B.C. From then until 1450 they become equally long again (Gills and Frank 1992). I did not, as Barceló says we should not, confuse them with the Kondratieff cycles since then, but I do not exclude the possibility of Kondratieff-length or even -type cycles within the longer ones. In this regard, a brief self-criticism: Strictly speaking, we should speak not of cycles but of *one* cycle with recurrent A and B phases whose turning points are endogenous. Failing the latter, we would do better to speak of “waves” (Frank n.d.).

Joffe seems to agree that there are correlations in the cyclical events reviewed here (and we can leave the matter of causation till later). More than a correlation, I found almost complete coincidence between the cycle phases identified by Gills and me (1992) and the periods independently distinguished by the Sherratts. Now they write that the search for a systemwide pattern of cycles

“may be a heuristic exercise” but then that this attempt is “inherently misguided,” although it has “its own logic.” Multiple choice! Check “All of the above”? The disagreements about cycle dating and phase naming of the other commentators mentioned above suggest that they are talking about cycles/phases that are both real and identifiable. I can only reply that cycles may well be questionable, but unless we ask the question we are unlikely to get an answer.

Chase-Dunn wants further clarification of cycles. Lamberg-Karlovsky, true to form, rejects them. In CA☆ comments on related topics, he has also complained of lack of “evidence” to Kohl (1978:479) and absence of “fact” to Zagarell (1986:427), from whom he has also sought “greater clarity.” If there is more or clearer evidence to support his denials of the existence of cycles, he does not offer it. Perhaps he wishes to invoke the above-mentioned archaeological truism for himself. Chase-Dunn, in contrast, cites evidence of cycle phases that coincide between the Mediterranean–West Asia and East Asia (but not India) since 430 B.C.; the graph in the paper he cites (Chase-Dunn and Willard 1993) shows the same since about 600 B.C., which not incidentally is when I claimed East and West Asia were united in a single world system. Chase-Dunn and Willard’s test of the dating itself yields only “mixed results,” and their construction of city-size *distributions* is doubly inconclusive. They disconfirm as many of Gills’s and my datings and identifications as they confirm, but their data begin only in 1360 B.C. and have such long time gaps that several cycles cannot be evaluated at all. Barceló and the Sherratts have reservations about “absolute” denominations of A and B phases, pointing out that an “A” may be only a “B+” and that phases in different places appear to go in different directions at the same time.

Chase-Dunn accepts cycles but asks whether to regard phases of economic growth and hegemony as alternatives or as complementary. Friedman also accepts cycles, albeit perhaps not systemwide ones, but expresses reservations about these and other components. His request for more specification in a “processual model” is, of course, acceptable, but his alternative is dubious. He argues that empire building is simultaneous with decentralization, both being associated with—and decentralization being a “cause of”—decline and crises of accumulation. He notes that I maintain the contrary, and the Mesopotamian, Chinese, and European cases he mentions have not [yet?] moved me to change my mind. Thus, my answer to Chase-Dunn is that economic and political expansion seem to complement each other (Frank and Gills 1992; Gills and Frank 1990, 1991). This is not necessarily to deny Kristiansen’s valuable suggestion that some peripheral growth follows central collapse.

The alleged world system: Too big or too small. For Edens and Kohl, who have taught me so much, there was a world system in West Asia, but it did not extend as far east as early as I suggest. McNeill, in contrast, detects a neglect of seaborne extensions of the world system still farther east, not to mention south to East

Africa. Here my inclination, also by reference to the truism about evidence, tilts toward McNeill. Algaze reiterates his perception of several world systems, to which I have already taken exception. Knapp finds my bounding of the world system too limited in the west because to his taste I do not pay enough attention to the Mediterranean and Europe. The Europeanists Kristiansen and the Sherratts do not, however, complain of neglect of their areas. It is therefore difficult to know what to make of the Sherratts' comment that the 3d millennium witnessed spatial core-periphery shifts but by the early 2d millennium Central Asia and the (eastern) Mediterranean had "bifurcated." Is this intended to suggest that they became *two* separate world systems until they were "united" again in the Achaemenid period? What is the rationale behind this suggestion? On present evidence it seems unlikely that the world system extended farther west and not farther east, especially until (as several commentators observe and I have already noted) the Phoenicians extended it into the western Mediterranean in the 1st millennium B.C. McNeill suggests that China, which I saw as incorporated into the world system by the middle of the millennium, was already its "core" before the end. Perhaps it was, if there was a single core, but this does not justify his rejection of my reservations about Gernet's Sinocentrism or his suggestion that the system danced to a Chinese beat. I was suggesting that China, Central Asia, and other parts of Eurasia were responding to a world-systemic beat, not simply or primarily a Chinese one.

Algaze's plea for several world systems of course contradicts all of the above. Unfortunately, Algaze also contradicts himself: His definition of a world system in terms of "a dynamic structure of relationships of interdependency, principally (but not solely) economic" is precisely what Gills and I used to delimit the world system in the Bronze Age. That "polities" A and C have relations with B and not with each other or with the world system as a whole is a caricature of my argument. It hardly qualifies A, B, and C (or, more concretely for Algaze and me, Egypt, Mesopotamia, and the Indus Valley) as separate "world-systems," much less so if we consider their relations in turn with Arabia, the Levant, Anatolia, and parts of Central Asia, not to mention the steppes north of the mountains examined by Chernykh. Unfortunately, no commentator refers to Chernykh's and my inclusion of this vast Eurasian expanse in the world system, and it remains unclear of what this absence of comment is evidence.

Of course, as the Sherratts point out, "cyclicity is not the sole criterion of systemic relationships," and I nowhere suggested that it was. But there is no reason, *contra* the Sherratts, that attention to cyclicity and the observation of cyclical "correlation" or "coincidence" over a huge area in Eurasia should "divert attention from rather more fundamental factors" such as the ones Algaze and I set out in principle and in practice. Rather, cyclical correlation is *prima facie* evidence, as Chase-Dunn writes, "that something is going on," that is, that "more systemic relationships" are at work, and it pro-

motes the search for them. A major case in point is my inclusion in one "integrated" world system of both the regions mostly south of the mountains studied by Edens, Kohl, Lamberg-Karlovsky, et al., and those mostly farther north—and east—reviewed by Chernykh. For Kristiansen, the demonstration of cyclical regularities in time and space is the "primary 'test'" of the idea of a single world system and its geographical and chronological limits. Of course, Edens is right in noting that not all contact need be systemic. But what pattern, degree, and/or evidence of interaction and interdependence, not to mention dynamic structure, would Edens and Algaze then accept for inclusion in one world system, and why should cyclical "correlation" not be among them?

Mechanisms of the world system: Unspecified or non-existent. Edens rightly finds insufficient specification of the mechanisms whereby interaction is propagated—something I said could be left for another time or to those better qualified (such as Edens himself). I hope that he will continue to provide such specification. Barceló rightly asks for consideration of the intensity of interaction and/or of the operation of these mechanisms. With Edens, Gosden laments that these mechanisms are insufficiently quantified; right again. (This was also, as Kohl [1978:488] noted, "the most frequent and fundamental objection" to his analysis.) I applaud all appeal to evidence for the mechanisms and intensity of interaction, direct and indirect—such as the *prima facie* evidence of cyclical connections and of Eden's suggestion of "sloping horizons" of their propagation from west to east. Chase-Dunn rightly demands more attention to differences in the spatial dimensions, especially of political/military and trade networks. He complains that I "never made clear" that the latter are typically larger than the former and, specifically, that economic relations between Egypt and Mesopotamia antedated political ones by at least 1,000 years. I am sorry, since I devoted a number of pages to this theme, and Chase-Dunn's clearer statement only supports my contention that the effective world system emerged early on and was much larger than political ones such as those in terms of which Algaze identifies "world-systems."

Thus, the "incongruences between settlement pattern, politics, and economy" that Edens refers to require much more evidence and analysis to refine the identification, not to mention the interpretation, of possible cycles (for which I never proposed a single measure, as he claims). Friedman, however, mentions three very similar processes—in commercialization, politics, and social existence—but implies, contrary to Edens, that they are congruent and synchronous. Though culture and ideology are now a major interest of Friedman, his lone reference to them during the "axial age" is consistent with my observation (Frank 1978a), which he disputes, that, at least more recently, paradigmatic advances in science, technology, philosophy, and the arts are generated by B-phase economic crises.

McNeill suspects an urban bias. I think that there is less bias than lack of the clarity on my part. Cities go with complex society, population growth, long-distance

trade, center-periphery structure, imperial and colonial ventures, war, etc. I did not “rely” on city-size data to identify cycle phases but reported their tests by other scholars, who use them because—notwithstanding doubts about their inclusiveness and reliability—they offer partly *prima facie* and partly substitute evidence for other indicators of connections which are less likely to have survived. Other commentators complain that I lend too much or the wrong weight to migration and war.

All of the above is contradicted and rejected in principle and in practice by Gosden, Gilman, Joffe, and, most strangely, Kohl. The same Gosden who laments the lack of a measure of world-systemic interaction also alleges that in principle there is no point in pursuing one for the Bronze Age. Gilman prefers to confine us to primitivist-formalist analysis of small societies and areas, but, generally speaking, this seems like a move in the wrong direction. (It would be especially difficult for me after having [mistakenly?] invoked formalism in the second issue of CA over 30 years ago [Frank 1960] and rejected both that and substantivism in these pages over 20 years ago [Frank 1970].)

Joffe wants us in practice to concentrate on the local and the contingent and eschew all else, so it is not surprising that he recommends Mann’s (1986) analysis. I have already explained elsewhere why I do not find Mann’s “politics in command” approach commendable. In recent times, both in theory and in praxis, this Maoist approach has been overwhelmed by world economic forces, along with the politics and ideology of Mikhail Gorbachev and Ronald Reagan (Gills and Frank 1990, 1991; Frank and Gills 1992; Frank 1993). Power is certainly one of the mechanisms that Edens would have us study, but to lend priority, let alone exclusivity, to political power and disdain economic force as it underlies the exercise of power is false consciousness that has led to grief both in recent history and in and about the Bronze Age.

Surprising, however, is Kohl’s emphatic rejection of a “world system.” I noted his repeated caveats and expression of reservations about the “use and abuse” of world system theory and his recognition of the need to “adapt” Wallerstein’s theory for use in earlier times, but he still seems to be wriggling in its straitjacket. This has not prevented him from doing much to advance the analytical and practical use of world system theory for the Bronze Age. He concluded his own CA reply as follows (1978:489):

One purpose of this study of long-distance trade in southwestern Asia was to show that even the earliest “pristine” example of state formation cannot be explained entirely as an internal process of social differentiation but must be viewed partly as a product of a “world economy” [*sic*: no hyphen!] at different levels of development which stretched at least from the Nile Valley and southeastern Europe in the west to Soviet Central Asia and the Indus Valley in the east.

Since then there have been some changes. For instance, Central Asia is no longer “Soviet,” and in the English version of a book by a formerly Soviet archaeologist (for which Kohl wrote the introduction) we can now read that “the world system has turned out to be far more extensive than appeared earlier” (Chernykh 1992:304). It is therefore difficult to take that Kohl now says flatly that the world system idea is *not* useful. Other commentators, such as Knapp, complain that my reliance on Kohl is excessive. Perhaps!

Accumulation of capital and migration: No—but yes. Barceló accepts everything in my model except capital accumulation. The Sherratts, on the contrary, find the discussion of capital accumulation well taken but take exception to much else. Chase-Dunn “agrees” that “markets and capitalism have existed for millennia,” though I deliberately made no reference to “capitalism.” Schneider (1977) argued for the importance of trade in luxuries and precious metals, and Ekholm and Friedman (1980), among others I cited, saw capital accumulation in the ancient empires. But Friedman now proposes a “refinement” of capital accumulation through abstract wealth, regarding which I in turn would like clarification and/or examples. In any case, it seems that even more recognition and attention is merited by the role of trade in luxuries and, of course, in precious metals in capital accumulation (Gills and Frank 1990, 1991). Chase-Dunn also finds an undefined “distinction between capital accumulation and capitalist accumulation” valuable for understanding the differences in the world system before and after A.D. 1500, which brought a “fundamental transformation.” Since it was not my topic here, I did not mention that I no longer see any such transformation (Frank 1991b, 1993). I am also accused of excessive attention to major migrations; I confess to the sin of excessive omission instead.

World system analysis: Not as far back as the Bronze Age—but as far back as the Neolithic. Most commentators said or implied that “modern” world system analysis is not applicable as early as I want to use it, and Lamberg-Karlovsky thinks that nothing so modern is applicable to earlier times at all. To the contrary, Gosden (the same one who questions the applicability of this idea in the Bronze Age?) wants us to push it much farther still, to the Neolithic. He is probably right about reading world (system) (pre)history back as far as we can, though the Neolithic may remain beyond our reach. What we need, of course, is evidence, if not measures, of world system interaction and its mechanisms. Wilkinson (1987, 1993, and elsewhere) traces *and maps* “central civilization” (what Chase-Dunn calls the central world system) back to interaction between Egypt and Mesopotamia in 1500 B.C. Gills and I adduced indirect cyclical evidence back to 1700 B.C. and some other evidence beyond that. Pursuing the same, I have sought to bring more evidence to bear back to 3000 B.C. and pointed to admittedly scattered additional evidence for the 4th and perhaps 5th millennia B.C. Chernykh of course has access to and command of vastly more evidence for those millennia and suggests that the world

system has turned out to be not only far more extensive but also much earlier than we thought. Was there one or not?

Reification vs. diffusionism. Other comments rise to the stratospheric level of metatheory in discussing the method in my madness: (1) Barceló sees a model, of which he approves, and Kohl says that this is useless and of course unwarranted reification of the "world system," which really exists only in our heads. (2) For Edens "world system" involves circularity of definition because (to translate him into plain English) you know one when you see one. (3) A "world system" is an empirical generalization, according to Knapp, who for reasons unknown rejects this much-used inductive method as inadequate for theory construction. (Is an "empirical generalization" different from an "empirical law," which Barceló contrasts with a "model"?) (4) "World system" is really a latter-day version of old-fashioned and long-since-discredited diffusionism, according to Algaze and Joffe, the former nonetheless giving the effort passing marks and the latter failing ones. With this multiple choice, it is hard to know if I should check none of the above or perhaps 3, since I do rely partly, and quite legitimately, on this inductive method in bounding the world "system" and dating its "cycles."

I don't deny that in my head there is some world system "model," though it is less clear what is in the head of Kohl, who has written so much about it. But *contra* postmodernists, I am well aware of the difference between my head and the rest of the world and do not confuse the contents of the former with the real world. However, I do not deny that there *is* a real world out there and that, contrary to Algaze, Joffe, and others (now including Kohl?), during the Bronze Age this reality included widespread interconnections and mechanisms, perhaps too dimly perceived or understood, to which it is useful to give the name "world system." Using this handy label therefore need not involve any circularity. As McNeill says, call it what you will (William McNeill [1963] and Marshall Hodgson [1974] called it the "ecumene"), "it" certainly existed and was "until quite recently the cockpit of world history."

It seems equally clear to me that this inductive method is a far cry from any diffusionism, discredited or not. Moreover, since these commentators have brought it up, is diffusion to be denied? The Swedish archaeologist Sinclair (Sinclair and Pwiti 1988, Sinclair et al. n.d.) and his team of several dozen African graduate students digging up East Africa are hell-bent on demonstrating everything they find to be pristinely "indigenous," because they explicitly want to counter the false charge that Africans are and have always been unable to do anything for themselves. But why should recognition of interconnections have to mean (one-way) diffusion to Africa, throwing out all the Indian Ocean connections? I can only reiterate what Kohl (1978:488) has already said in response to a similar charge: "Today, the danger lies in ignoring or neglecting the *fact* of diffusion, not misusing it as a *deus ex machina* type of explanation or as a disguised form of racism." Kohl wrote before the

diffusionist “Afrocentrism” (e.g., “Beethoven was black”) had gained some adherence and notoriety in the United States, and at least the pristine antidiffusionism of Sinclair et al. unwittingly serves to debunk this tendency. As Kohl has shown in papers on archaeology and politics in Transcaucasia and elsewhere, the “evidence” dug up or if necessary invented by archaeologists is now invoked by politicians to “prove that this land is mine” and needs “ethnic cleansing.”

Evidence: Yes, no, and maybe. The range of rules of evidence attributed to me is wide. If there were no evidence, as Gilman suggests, neither my sources nor I would have gotten even this far, and those who adduce contrary evidence would have had nothing to criticize. Additional evidence is now supplied by UNESCO’s 15-years-in-the-making first volume of the *History of Civilizations of Central Asia: The Dawn of Civilization, Earliest Times to 700 B.C.* (Dani and Masson 1992). It refers to “accumulation of wealth” (p. 232), “large-scale investment” (p. 196), and many extensive east-west as well as north-south interregional links between West, South, Central, and East Asia (pp. 196–338). Although none of the contributors mentions a “world system” or “cycles,” several chapters on the Bronze Age present evidence in support of the cycle dating proposed in my essay, especially for the 3d millennium. The chapter on Iran and Afghanistan remarks on a higher growth rate, urban expansion, and accumulation with increasing urban-rural and “hegemonic-centre”–periphery disparities during the first half of the millennium and generally declining ones during the second (pp. 196, 204, 206). There is also additional evidence for my shorter cycle dating: The provisional A phase 3000–2800 B.C. witnessed “considerable increase in size of each regional centre . . . [and] maximum expansion,” especially in the Helmand Valley (p. 199). Similar expansion occurred in Mehrgarh until it was abandoned before 2600 (pp. 254–55). Support for the next A phase comes from eastern Iran and Afghanistan in 2500–2300 B.C., when there was construction at Mundigak and Turang-tepe in “the phase of most rapid expansion of urban centres and the most extensive territorial integration” (p. 207) whose “timing and stages . . . apparently corresponded to, or just preceded, those of the Indus civilization” (p. 210). In Khorasan and Transoxania also, the “pinnacle of local culture” was reached in 2300 B.C. Then, however, in what I termed a B phase after 2300 B.C., “crisis affecting the whole basin” came “towards the end of the third millennium B.C.” (pp. 212–13). “Urban civilization suddenly collapsed. It lost all of its features of centrality, and most of the cities were sharply reduced or abandoned in the space of a few years” (p. 215). Indeed, elsewhere as well “after the year 2200 . . . the urban system begins to deteriorate and there is radical and rapid decline in the large centres in all the enclaves of Central Asia. None of the explanations proposed so far successfully link up the numerous conditions of the archaeological evidence over such a wide area” (p. 200). The cycles I have proposed are an attempt at such an explanation, and although it may not yet be “successful,” why deny the evidence?

It is not true, then, that there is no archaeological evidence (Lamberg-Karlovsky), though there is certainly less of it than we would like. Chernykh surveyed such evidence; I hope that the Southwest Asianist and Europeanist commentators will uncover as much or more in the areas of their own specialization. And why not use secondary sources (Muhly)? To begin with, just what makes a "primary" one primary? Moreover, I was very explicit that this article was not to present any new evidence but to relate the observations of others and derive some "empirical generalizations" therefrom. In 1966, I wrote a history of Mexican agriculture from 1521 to 1630 which reinterpreted two renowned scholars' primary evidence and challenged their local institutional explanations and instead explained the same (better, I argued) as local adaptations to world economic relations, forces, and cycles. I sent the draft to one of these authors, and he replied that it should not be published because it relied only on secondary sources. I followed his bad advice for 13 years. When I finally submitted it "as is" and it was published (Frank 1979) by Cambridge University Press, the same authority reviewed the book and said that it should not have been published because by now my thesis was old hat. Indeed it was, insofar as in the intervening years primary and secondary scholarship had completely reversed, the previously orthodox interpretation of the data and vindicated my thesis. I insist on the necessity of evidence, but Thomas Kuhn's work on paradigm shifts has shown that they do not emerge from confirming old theses with more and more primary evidence. Instead, paradigms are changed by offering a different perspective, even for the collection and use of evidence. Muhly is welcome to reject, modify, or even collaborate in this task.

According to Gilman and Joffe, using secondary sources is OK but not quoting them only where and when convenient. Really? According to the Sherratts I have drawn on "relevant, reliable authorities." It would be improper indeed if I had suppressed the authority of contrary *evidence*, but that is not the charge. Gilman and Joffe want me to cite writers whose paradigm is different from mine and who therefore may not even consider the kind of evidence I adduce. Of what use might that be?

Tainter (1989), for example, with whose neglect Joffe chastises me, reviews dozens of explanations grouped into six categories of theory of the collapse of complex societies before offering his own. Many of these might indeed be useful in identifying B phases. Unfortunately, *all* of them attribute collapse to internal factors alone. Even when they observe several empires collapsing at the same time (in a B phase?), these scholars are not moved to inquire whether there might be a trans-"societal" or common cause, as Teggart (1939) did when he examined "correlations in historical events" in Rome and China. Teggart did not, and *contra* Joffe I do not, equate correlation with causation. However, all statistical tests and therefore many scientific methods are based on the probability premise that where there is smoke, there is at least some presumption of fire. Therefore most scientists who find high correlations among

events are led to suppose that there may be some mutual or common causation into which we would do well to inquire. Why does Joffe not only sit back—for he admits to correlations—but insist that others do so too?

Braudel, of course, confines himself to a much later period and concentrates on the Mediterranean, so there seemed little reason to bring him into the Bronze Age. However, since Joffe insists, in *The Perspective of the World* Braudel (1982) distinguishes between several “world-economies” and one “world economy” and sets out to use Wallerstein’s model of the former, but time and again he demonstrates the vital and even systemic interconnections among the supposedly “autonomous” European, Russian, Ottoman, Indian, Chinese, and Southeast Asian “world-economies” in a single world economy (albeit in the much later period he covers, since A.D. 1100). Citing chapter and verse from Braudel, alas only a secondary source, I have recently demonstrated the same (Frank 1993).

According to Knapp, my sources, evidence, and even acknowledgments give short shrift to Europe and the Mediterranean. However, Kristiansen comments that “European Bronze and Iron Age researchers can recognize the shifts identified,” and the Sherratts also do not remark on such neglect. I certainly did not intend to neglect this area, and I acknowledged the help of Kristiansen and Sherratt in the preparation of the article; but for Knapp’s sake I am glad to do so once again and to add my thanks for their further help in their comments above.

Barceló notes a failure to use some very recently published (secondary) sources; Joffe complains instead that I rely too much on material that is not yet published. I use what I can get.

Usefulness: No—and yes. Is there any point to this? Again opinions are diverse. For “cons” it is far too general and vague, not useful, misleading, simplistic, highly strained, unacceptable as serious scholarship, and the work of an agent provocateur. “Pros” call it an important contribution, a brilliant portrait, a salutary serious attempt, stimulating, food for thought, a landmark, a valuable platform for discussion, and explicitly provocative (is this the same as “the work of an agent provocateur”?). Why not compromise and evaluate this modest effort by splitting the difference and settling on the golden—but more humdrum—middle? Perhaps we can further such compromise by noting that some of the entries on the credit and debit sides of this ledger are by the same person. Besides, Algaze attributes heuristic value to “world system” but thinks that Kohl uses it better, while Kohl now finds it misleading. Even if, according to Edens, it overreaches, “world system” analysis does seem to lead somewhere after all—alas, not where Joffe and others would choose but apparently where Barceló, Chase-Dunn, Friedman, Kristiansen, and perhaps McNeill and the Sherratts (not to mention probably Chernykh, who was unable to comment, and of course myself) want to go.

Finally, I did refer to a “central world system,” combining my terminology with Wilkinson’s because, as I

thought I had clarified, Chase-Dunn (and Hall [1991]) had told me to. However, I never referred to the "total accumulation" that Joffe attributes to me, nor is there any "basic Marxian notion of the ruling class" at the core of my analysis. On the contrary, I went to considerable lengths to show how the powers of ruling classes have always been seriously limited by world-systemwide competitive and cyclical forces beyond their control. It is Joffe and not I who argues that ruling-class politics is in command. It may be overreaching to put the Bronze Age in the same light as our own, but, as Kohl (quoted above) has said, it is impossible to explain "pristine" state formation entirely as an internal process of social (class) formation—in a "world economy" that he saw as stretching "at least" from the Nile to the Indus. Does the same not apply to the collapse of the complex (class?) societies reviewed by Tainter? How illuminating is it, then, for Joffe and others to insist, against the evidence presented above and elsewhere, that Bronze Age society was governed only by locally contingent politics and ideology in command? Is this not a vain attempt to maintain ideology in command here and now?

In closing this reply but I hope not the debate, I would like to agree with Knapp that my effort would have benefited from archaeological coauthorship. And if by chance this is an elegant way of saying that it would have benefited from exclusively archaeological authorship, I would agree—so much so that before writing the first word I suggested to Chris Edens and Phil Kohl that *they* do it. They did not choose to do so, and so I used my letter to them (suggesting revisions to their 1993 paper) as a point of departure. I only sought to follow their lead. Beyond that, all the usual disclaimers apply, and so do my thanks again to all and sundry.

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Calendar

1993

- October 27–31. American Folklore Society, Annual Meeting, Eugene, Ore., U.S.A. Write: AFS, 1703 New Hampshire Ave. N.W., Washington, D.C. 20009, U.S.A.
- November 4–7. American Society for Ethnohistory, Annual Conference, Bloomington, Ind., U.S.A. Write: Program Chair, Raymond J. DeMallie, or Local Arrangements Co-Chairs, Douglas R. Parks and R. David Edmunds, American Indian Studies Research Institute, Indiana University, 422 N. Indiana Ave., Bloomington, Ind. 47405, U.S.A.
- November 11–14. Chacmool Conference, Calgary, Alberta, Canada. Theme: Cultural Complexity in Archaeology. Write: The 1993 Conference Committee, Department of Archaeology, University of Calgary, Calgary, Alberta, Canada T2N 1N4.
- November 17–21. American Anthropological Association, 92d Annual Meeting, Washington, D.C. Write:

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AAA, 1703 New Hampshire Ave. N.W., Washington, D.C. 20009, U.S.A.

1994

- January 5–8. Society for Historical Archaeology and Advisory Council on Underwater Archaeology, Annual Meeting, Vancouver, B.C., Canada. Plenary theme: Science and Technology in an Interpretation and Presentation of the Historic Past. Write: Program Chair, Department of Archaeology, Simon Fraser University, Burnaby, B.C., Canada V5A 1S6.
- March 21–24. International Conference on Ethnic Traditional Culture and Folk Knowledge, Moscow, Russia. Write: Organizing Committee, International Conference, Institute of Ethnology and Anthropology, Leninsky Prospect 32A, Moscow 117334, Russia.
- April 5–11. William Robertson Smith Congress, Aber-